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CONTENTS OF No. 19.

	PAGE
Tricycles in 1882. By John Browning	397
The Great Pyramid. By the Editor	398
Photography for Amateurs. By A. Brothers, F.R.A.S.	400
The Crystal Palace Electrical Exhibition. Fifth Notice	401
Our Ancestors.—II. The Celts. By Grant Allen	402
Notes on Rowing. By an old Club Captain	403
The *Spectator's* Comet	404
Newton's Views as to a Menacing Comet. By A. C. Ranyard	404
Intelligence in Cats	405
Wood-Gas	405
For Next Week	405
Meteorological Reports	405
Omissions and Corrections	405
Zodiacal Map	406
CORRESPONDENCE:—Vegetarianism—Intelligence in the Dog—Electrical Images — Darwinism — Arranged Squares—Ghosts—Telephone, &c.	407–409
Queries	410
Replies to Queries	410
Answers to Correspondents	411
Our Mathematical Column	413
Our Chess Column	415
Our Whist Column	416

TRICYCLES IN 1882.

BY JOHN BROWNING

(Vice-President of the Tricycle Association).

ABOUT five years since the first modern tricycle was built on the lines of the spider bicycle. This new build, known as the Lever Coventry, was regarded with amused curiosity by the general public, and with utter contempt by most bicyclists; yet it wanted only a strap-brake—that is, a band working on a drum—to make it a thoroughly safe and efficient tricycle.

Soon after this, Starley, the Stephenson of the tricycle, who contrived the Lever Coventry, produced the Salvo. This machine had a rotary motion obtained by means of cranks, as in the bicycle, which was communicated by means of a chain to the driving wheels.

But the great peculiarity of the Salvo was that both wheels were driven so long as the machine was moving in a straight line, while either wheel was free to stand still, or even run backwards, when the machine was turning. There are now at least 150 tricycles, or rather machines known by that number of different names, but these are principally of three or four types, and all those belonging to the largest and possibly the best type, are modifications, sometimes only in name, of the "Salvo."

Less than two and a half years ago the two principal tricycle clubs of the metropolis, the London and the Finchley, arranged a fifty-mile ride for the Tricycle Championship. This was ridden over the hilliest road out of London in four hours fourteen minutes. The rider, Mr. Lacy Hillier, is the amateur champion of the bicycle and tricycle. The machine he rode is known as the Humber, and it was brought out for this ride. Looked at sideways, it has the appearance of a bicycle; but it may be described as a bicycle with two front wheels. The rider sits on a saddle between them, and steers by means of a cross-bar which turns them both. This is probably still the fastest tricycle, but it requires some practice to ride it at all well. It is not a good luggage carrier, it will not turn easily or in a small circle, and it requires careful riding, particularly down hill. For these reasons it is not so generally adopted as its good qualities deserve it should be.

About twenty or thirty novel tricycles have been introduced this year. Of these about a dozen possess great originality, and three or four considerable merit. The Rucker, the National Arms Company's National Tricycle, the Improved Omnicycle, the Improved Devon, the New Rotary Coventry, and the Monarch, are, in my opinion, the best machines, the Monarch being the most original brought out this season. The whole of the machines I have named, with the exception of the Coventry, are double-drivers. Of these the Rucker and the Monarch have the small steering wheel behind, and thus are perfectly open in front. I do not say that such machines are by any means less liable to accidents than those which have the steering-wheel in front, but if accidents should occur, they are not likely to have such serious results, as the rider may jump out or fall out of the machine without falling on to or being caught by the steering gear or front wheel.

About two years since, most of the tricycles which were driven by means of a chain were geared-up—that is, the driving wheels were made to go round faster than the pedals. In some letters I then wrote to the *Cyclist*, I asserted that this was a mistake. Now, most of the best riders agree that tricycles should be geared down—that is, the pedals should go round faster than the wheels. In such an arrangement, of course, power is gained and speed is lost. When the gearing down is carried to a great extent, the pedals make two revolutions for one revolution of the driving-wheels. A machine so geared can be ridden up a steep hill easier than a bicycle. The chain is a source of trouble in a tricycle, though it has been improved of late. It is liable to stretch and so slip over the cogs, and I believe, occasionally it has brought a machine to a dead standstill for an instant, by not passing round with the cogs freely. The result of this has been that the rider has been thrown out of the machine. Some tricycles have two chains—one to each of the driving-wheels; these, with the exception of the Monarch, are the only true double-drivers, and are the best for mounting hills. The machines which profess to be double-drivers, through the inter-mediation of what is called a balance action, are double-drivers only so long as the resistance to both wheels is the same. As soon as one wheel experiences more resistance than the other, it ceases to drive, just when its driving power is the most wanted to overcome an obstruction.

Probably the best size for the driving-wheels of a tricycle is from 48 in. to 50 in. diameter. If smaller than this they rise with difficulty over any small inequalities in a rough road. If larger, unless very stoutly made, they are weak, and if strong they make the machine unnecessarily heavy. The weight of tricycles with from 48 in. to 50 in. wheels varies from 75 lb. to 120 lb., but very few are less than from 95 lb. to 98 lb. This weight is more than it should be for machines intended for riders weighing from 8 to 10 stone.

Manufacturers ought to make machines of different degrees of strength and weight. At present, all machines are, as a rule, strong enough to carry a man who weighs 16 stone. There are, however, two new machines this season which are excellent in this respect. The National Tricycle Co.'s machine weighs only 70 lb., and the Monarch only 63 lb. Until the Humber Tricycle was produced, tricycles were of very inferior workmanship to bicycles; but that machine, the Cheylesmore, the Coventry Rotary, and the Premier are now of first-rate excellence.

Generally speaking, the worst points of a tricycle, both as regards contrivance and workmanship, are the pedals, and

these require to be the best. The whole weight of the rider is thrown on these in riding up-hill, and the friction of badly-made bearings is very great. I have seen such bad workmanship in the pedals of otherwise fairly-well-made machines, that an ironmonger would have been ashamed of such work in a set of the commonest kitchen fire-irons.

At present there is only one good brake. This is a circular or semi-circular band of steel, which, by means of a lever, can be made to clasp a drum. It is in a few instances, and should always be, applied by means of two straps and drums and to both wheels. If one band or drum were to give way in descending a hill, the other would then suffice to prevent an accident.

Great improvements have been made recently in double tricycles, or, as they are generally called, Sociables. Last year the Sociable Salvo was the best machine of the kind, but now the Premier Sociable is both lighter and a far higher class of work. Machines of this kind, to carry two riders side by side, need not weigh nearly double the weight of two single machines of the same make. From being much wider, they are safer than single machines, and they run lighter than single machines when they are driven by two practised riders. A better speed can be obtained on them than on a single machine. A good rider can carry a lady on the seat beside him with very little assistance from his fair companion. Indeed a problem has been proposed: Given a lady and gentleman driving a Sociable, to find the amount of the pressure the lady puts on the pedals?

But either with a lady or a gentleman, riding on a Sociable is by most experts admitted to be the most enjoyable form of this the most enjoyable of all sports.

THE GREAT PYRAMID.

By the Editor.

WE have seen that the Great Pyramid is so perfectly oriented as to show that astronomical observations of great accuracy were made by its architects. No astronomer can doubt this, for the simple reason that every astronomer knows the exceeding difficulty of the task which the architects solved so satisfactorily, and that nothing short of the most careful observation would have enabled the builders to secure anything like the accuracy which, as a matter of fact, they did secure. Many, not acquainted with the nature of the problem, imagine that all the builders had to do was to use some of those methods of taking shadows, as, for instance, at solar noon (which has to be first determined, be it noticed), or before and after noon, noting when shadows are equal (which is not an exact method, and requires considerable care even to give what it *can* give—imperfect orientation), and so forth. But to give the accuracy which the builders obtained, not only in the orientation, but in getting the Pyramid very close to latitude 30° (which was evidently what they wanted), only very exact observations would serve. Indeed, if a modern astronomer, knowing nothing about the Pyramid, were asked how the thing could be done without telescopic aid, he would be apt to say that no greater accuracy than (for instance) Tycho Brahe obtained with his great quadrant at Uranienburg could have been secured. Now, the orientation of the Great Pyramid approaches much closer to exactness than the best observations by Tycho Brahe with that justly-celebrated instrument.

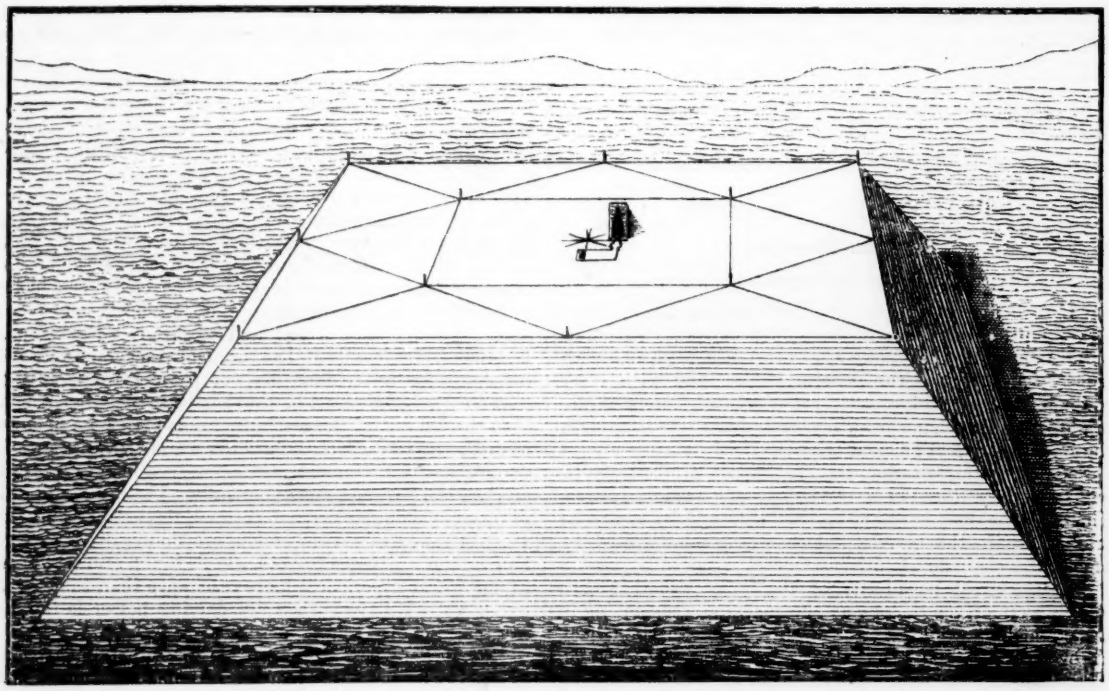


Fig. 1. The Pyramid Observatory, showing the object-end of the great observing tube.

Seeing this, and observing that the ascending and descending passages are just such as the astronomer would make to secure such a result, we may accept, without a particle of doubt, the belief that they were made for that purpose.

Then we saw that the features of the Great Ascending Gallery were not such as would be essential, or even desirable, to increase or maintain the accuracy of the orientation, as layer after layer was added to the Pyramid, but are precisely such as would be essential if the Pyramid was meant to subserve (as one, at least, of its objects) the purpose of an observatory.

But persons unfamiliar with astronomy will say (several have said so in letters addressed to me), This great ascending gallery would only enable astronomers to observe stars when due south, or nearly so, and only those which, when due south, were within a certain distance above or below the point towards which the axis of the Great Gallery is imagines that to be the chief observing instrument. The comparatively unobtrusive transit circle seems far less important. But the time observations, which are far and away the most important observations made at Greenwich, are all made, or at least, all regulated, by the transit observations. So are the observations for determining the positions of stars.

When the equatorial is used to make a time or position observation, it is used as a differential instrument, it is employed to determine how far east or west a star may be (theoretically, how much it differs in right ascension measured by time) from another; and again, to show how far north or south a star may be (theoretically how much it differs in declination) from another, whose right ascension and declination have already been determined by repeated observations with the transit circle. Similarly, the altitude and azimuth instrument is used in direct subordination to the transit circle.

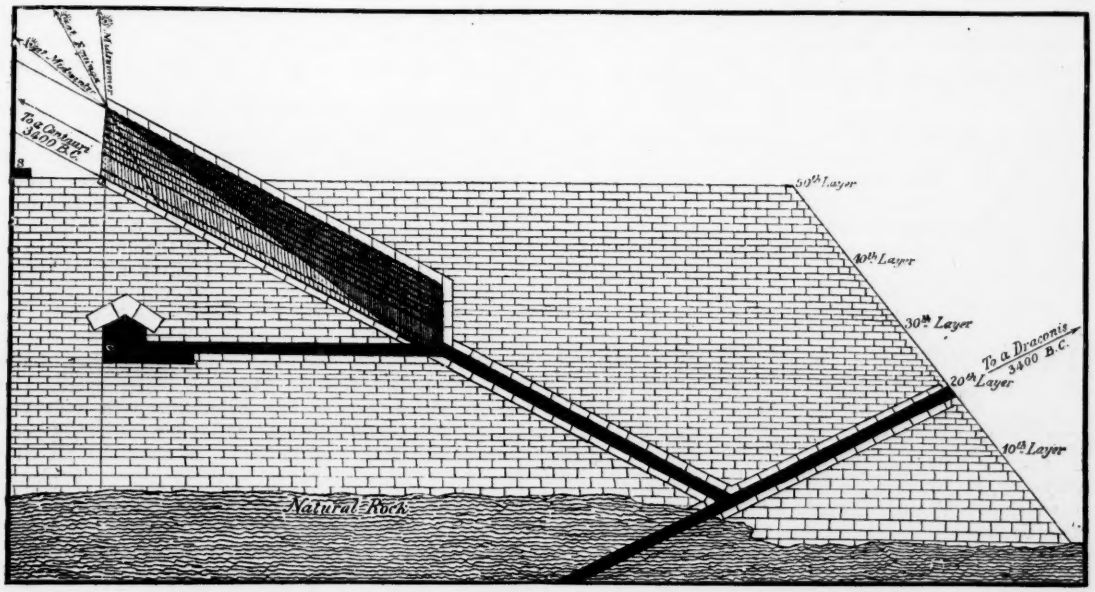


Fig. 2. Vertical Section of the Pyramid Observatory through the plane of the passages and gallery, showing the range of view of the great observing tube.

directed. Were all the other stars left unobserved? And again, we know that the Egyptians, like all ancient astronomers, paid great attention to the rising and setting of the heavenly bodies, and especially to what was called the heliacal rising and setting of the stars. In what way would the Great Gallery help them here?

Now, with regard to the first point, we note that the chief instrument of exact observation in modern observatories, the one which, as it were, governs all the others, has precisely this quality—it is *always* directed to the meridian, and has, indeed, a very much narrower range of view on either side of the meridian than the Great Gallery had. And though it is indeed free to range over the whole arc of the meridian from the south horizon point through the point overhead to the north horizon point, it is mainly employed over about that range north and south of the celestial equator which was commanded by the Great Gallery. The visitor at Greenwich sees the great equatorial, and

The astronomers who observed from the Great Pyramid doubtless made many more observations off the meridian than on it. They made multitudinous observations of the rising and setting of stars, and especially of their heliacal risings and settings (which last, however, though we hear so much of them, belonged *ex necessitate* to but a very rough class of observations). They no doubt often used astrolabes and similar instruments to determine the positions of stars, planets, comets, &c., when off the meridian, with reference to stars whose places were already determined by the use of their great meridional instrument. But all those observations were regulated by, and derived their value from, the work done in the Great Ascending Gallery. The modern astronomer sees that this was the only way in which exact observations of the heavenly bodies all over the star-sphere could possibly have been made; and seeing the extreme care, the most marvellous pains, which the astronomers of the Great Pyramid took to

XUM

secure good meridional work, the astronomer recognises in him a fellow-worker. He says, with the poet :—

> I am as old as Egypt to myself,
> Brother to them that squared the Pyramids:
> By the same stars I watch.

And now consider what was this great observatory of ancient Egypt—the most perfect ever made till telescopic art revealed a way of exact observation without those massive structures. A mighty mass, having a base larger than the square of Lincoln's Inn, rising by just fifty layers to a height of about 142 feet, and presenting towards the south the appearance shown in Fig. 1, where the mouth of the Great Gallery is seen opening southwards, and the lines are shown which have been already indicated as "observing directions" in the picture on p. 315. The Pyramid observatory is shown in section in Fig. 2. It will be noticed that the successive layers are not of equal thickness. There are just fifty between the base and plane of the floor of the King's Chamber. The direction lines for the mid-day sun at midsummer, midwinter, and the equinoxes are shown; also the lines to the two stars, Alpha Draconis and Alpha Centauri, are given at the subpolar meridional passage of the former and the meridional passage of the latter, at the date when the descending and ascending passages thus commanded both these stars. Within fifty years or so on either side of this date, the Pyramid must, I should think, have been built. The later date when Alpha Draconis was at the right distance from the Pole, 2170 B.C.,* is absolutely rejected by Egyptologists—not one being ready to admit that the date of the Pyramid King can have been anywhere near so late.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

AMONGST the readers of Knowledge must be a large number to whom the production of a photograph is a mystery, and there must be many who desire information which would enable them with little trouble and expense to become so far expert, that although in some respects the mystery will be as great as ever, they may practice the art with some success.

Less than fifty years ago, the only means we had for recording the appearance of natural objects was by drawing by hand, assisted in some cases by the *camera lucida*, an instrument which enabled the artist to have a correct outline of the object, whether portrait or landscape, but all detail of light and shade had to be filled in by hand in the ordinary way. The black profile portrait, or *silhouette*, is an example of what the photographer has superseded.

It is proposed, in a series of short papers, to give an outline of the history of photography, and details of some of the processes which will be sufficient to enable anyone to take negatives and to print from them. It must be remembered, however, that, although the processes are not difficult to master, some care and skill are required if the results are to be of any artistic value.

Without going deeply into the history of the subject, it will be sufficient to say that one of the chemical substances now commonly used in photography was known in the sixteenth century, and an alchemist, named Fabricius, found that *luna cornea*, as chloride of silver was then called, was so affected by light that an image produced by a lens became improved in light and shade on the surface prepared with the *luna cornea*. This must be considered as the first discovery of photography, but the discoverer failed to see the importance of it, and it was not until nearly two centuries had elapsed that the curious effect caused by light on the chloride of silver was re-discovered by Scheele in 1777. This chemist also failed to see the value of what he had observed, and it was not until 1802 that experiments were made at all analagous to what we now call photography. In the year last-named, Thomas Wedgwood, assisted by Sir H. Davy, produced pictures on white leather and paper, but, as no means were known by which the images could be fixed, none of these early photographs exist.

The process of photographic printing now in universal use, is based on these early experiments of Wedgwood and Davy, and it may be interesting to repeat an experiment, to show what kind of pictures they produced. Take a piece of smooth writing-paper, float it in a solution of common table salt (sodium chloride) for a minute or two, then hang it up to dry, or dry it by the fire. When dry, float the paper on a solution of silver nitrate, say 30 grains to the ounce of water (rain or distilled water should be used), or the silver solution may be evenly brushed over the surface with a large camel's-hair brush, or by means of a glass rod, so as to avoid using a large quantity of silver solution to float the paper upon. The prepared paper may be dried by the fire, or hung up in a darkened room or cupboard. When quite dry, a leaf, piece of lace, or any other suitable object may be placed on the paper, then covered with a piece of glass to keep paper and object in close contact, and then placed in sunlight. As soon as the paper is completely blackened it will be found that the picture of the leaf or other object will be printed on the paper as a negative—that is, the dark parts will be white, and *vice versâ*. The paper still remains sensitive to light, and, of course, must be viewed only by weak daylight or by artificial light. This, then, is the reason why the earliest photographs are not now in existence. Later, those *photoganic drawings*, as they were termed, were immersed in plain water, by which the silver salt was partially removed, but the pictures were not properly fixed, and therefore were not permanent. In a future paper we shall see how perfect fixation is effected.

We have no record of advance in photography until 1814, when J. Nicéphose, or Niepce, succeeded in producing permanent figures in bitumen of Judæa; the time required rendered this process impracticable. About this time, and leading up to 1839, Daguerre was engaged in researches which resulted in the discovery of the process which bears his name; and this process for many years was most extensively used, and the pictures produced remain to attest the beauty and value of the method. Daguerrotypes are permanent if protected from the atmosphere, which tarnishes the silver surface on which the pictures are produced. This tarnish, however, can be removed by chemical means, and the picture remains on the surface as perfect as ever.

* Some may be disposed to reject a change which they may imagine displaces the Pleiades from the position which Professor Piazzi Smyth assigned to that interesting group at the date when he supposed the Pyramid was built. But there never was the least real significance in that position. If the mistaken idea entertained by many, and repeated by Flammarion, Haliburton, and others, that the Pleiades at their meridian shone down the Great Gallery at the very time when the Pole Star of 2170 B.C. shone down the descending Gallery, had been correct, there might have been some reason to be struck by the coincidence. But it should hardly be necessary to tell the reader what every astronomer knows, that the Pleiades never did or could shine down the Great Gallery, and in the year 2170 B.C. were thirty-eight degrees (!) north of that position.

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

FIFTH NOTICE.

THE Exhibition may now be considered practically complete. Marvellous progress has been made during the last few days, and the Palace is crowded with visitors. Swan's exhibit is magnificent, but as it is our purpose to consider next the various systems of incandescent lighting, we refrain from making any further reference to it this week.

Telegraphy is well represented, the lead being naturally taken by the British Postal Telegraph department. It is somewhat remarkable that this is the only branch of applied electricity in which any serious effort is made to exhibit a chain-like series of historic relics. The display, however, lacks many interesting objects, in consequence of the foreign Administrations not having loaned any of their apparatus.

The Post-office exhibits may be divided into two classes —historic and modern. In the modern collection, interest is chiefly centred in the Wheatstone Automatic Instrument, by which messages may be transmitted at the rate of 200 to 250 words per minute. It is used on circuits which are required to carry a large amount of work, but it is being gradually superseded on ordinary commercial wires by duplex and quadruplex apparatus. Its applicability to news work, that is, the transmission of long messages for newspapers, is unequalled by any other form of apparatus, and as this kind of work frequently amounts to about 500,000 words forwarded from the Central Telegraph office during a single night, there is little prospect of its being relegated to the "historic" collection.

Various other forms of telegraph apparatus are exhibited, including duplex apparatus, by which messages may be transmitted simultaneously in opposite directions; but the quadruple apparatus, by which four messages may be sent at a time (two each way), is not exhibited by the Post-office. The familiar single-needle instrument is shown, as well as more the complicated fast-repeater apparatus, for both single and duplex working. These latter sets are used where it is desired to increase the working capacity of long lines. Practically, the repeater halves the length of the line. For instance, suppose we have a wire from London to Glasgow, and find that through certain causes, due to our uncertain climate, we can only work at the rate of, say, sixty words per minute. A station about mid-way, such as Leeds, is asked to insert his repeater apparatus in the wire. He does so, and, by an automatic arrangement, the current which leaves, say, London, only goes to Leeds, where, by moving the soft-iron armature of an electro-magnet, another circuit is completed, and a current from the Leeds battery goes on to Glasgow.

The historic collection is very interesting, even to the least curious of visitors. It includes the oldest known piece of telegraph apparatus, viz., Ronald's electric telegraph, which was laid as an experiment in Sir Francis Ronald's garden at Hammersmith, in 1816. There are also specimens of the Morse type, cast in 1832, when it was supposed to be impossible for human fingers to manipulate the apparatus in such a way as to be able to distinguish between long and short signals.

One of the most interesting objects is the "Fossil" underground telegraph (1837). Fig. 1 is a diagram representing a section of this line; it consisted of a kind of triangular wooden rod, with five grooves, into each of which a copper wire, covered with cotton and pitch (for insulating purposes), was laid. The grooves were then filled up by strips of wood. When thus finished, it was laid underground. Such a line contrasts most remarkably with modern wires. It, however, serves its purpose here, and takes the mind back really a few years, but, apparently, many a century. The

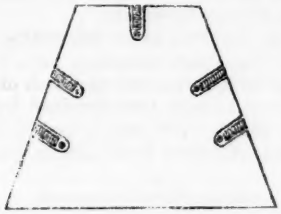


Fig. 1.

line was laid in connection with Cooke and Wheatstone's five-needle telegraph, which was used at Paddington and West Drayton in 1838. Fig. 2 represents the dial, or reading portion, of the apparatus. There were five magnetic needles, under the influence of as many coils of wire, each coil being in a separate circuit, including one of the wires represented in Fig. 1. The sending portion of the apparatus

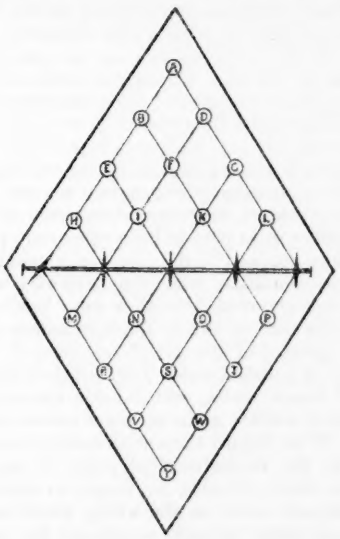


Fig. 2.

allowed of the current being sent from the galvanic battery in either direction, so that each needle could be deflected to the right or left. The letters of the alphabet (omitting those which were deemed of least importance or necessity) were marked on the dial as shown in the diagram. When it was required to telegraph a letter, two needles were deflected in such a way as to make both point to that particular letter. In the diagram the letter "B" is thus pointed out or telegraphed.

In 1840 the five needles were reduced to four, but two years later a most important change was effected. The instrument only had two needles, and instead of their being made to point out the letter, a code was arranged by which movements of either or both needles to the right or left a given number of times indicated the letter desired.

Another interesting and important relic is Bain's Chemical Telegraph (1850), in which the ordinary green ribbon used in the Morse inker is replaced by white paper,

XUM

prepared with yellow prussiate of potash (potassic ferrocyanide) and ammonic nitrate. The current, passing through a style pressing on the paper as it passed over a revolving metal wheel, decomposed the compound, and left a mark on the paper. There are very many other relics, far too numerous even to mention, including specimens of the earliest laid cables, insulators, etc.

What may be regarded as an interesting curiosity is a specimen of a Norwegian telegraph pole, which has been pierced through by woodpeckers in search of insects. The birds are supposed to have been deceived by the humming of the wires, a sound which may generally be heard near the poles, more particularly in still, calm weather.

OUR ANCESTORS.

By Grant Allen.

II.—THE CELTS.

WHILE the dark-haired and dark-skinned little Euskarians were living unmolested in the western coasts and islands of Europe—hewing patches out of the forest with their stone hatchets, building great barrows over their dead chieftains, and fighting among themselves from valley to valley, like the North American Indians of later days—a fairer and taller race was growing up unnoticed away to the east, among the great central table-lands of the Asiatic plateau. This fair-skinned, yellow-haired, and blue-eyed folk is known to us by the somewhat fanciful name of Aryans; and from it the chief conquering peoples of the whole Eastern hemisphere are derived. The Aryans spoke a language whose nature we can infer from the numerous modern dialects derived from it; and this language enables us in part to form some conception of the state of culture attained by the people who used it. In their earliest known condition, while they still all lived together among the high plains of Asia, they were hardly, if at all, superior in the arts of life to the Euskarians of Britain. They were ignorant of the use of metals, and armed only with weapons of polished stone. They fed their flocks like the semi-nomad tribes which still inhabit the same regions, and they tilled a little grain of some coarse cereal kind. Altogether, if we regard them with calmly impartial eyes, and not with the excessive filial piety of some German thinkers, we shall probably be forced to admit that the primitive Aryans were, on the whole, about as good and as bad as most other barbaric peoples at the same period of the world's history. Stronger than the neighbouring nations they certainly showed themselves to be, but wiser or better there is no sufficient reason to suppose that they were.

From their original Central Asian home, these warlike Aryans began to disperse themselves as fighting colonists on every side, probably some five or six thousand years since. One great branch, now speaking the Celtic variety of the common language, moved westward across the face of Central Europe; and its members spread themselves, long before the beginning of written history, over all the western coasts of the continent as a conquering and superior race. Though at first they were only armed, like the Euskarians amongst whom they came, with stone hatchets and flint-tipped arrows, yet, as they were tall, big-limbed, powerful men, while the Euskarians were comparatively short, squat, defenceless folk, they seem easily to have overrun almost the whole of what is now France, Spain, and the Low Countries, and to have established themselves, at least, as a rough aristocracy of chieftains among the conquered and servile Euskarian population. But in some places the Euskarians, and their kinsfolk the Ligurians and Aquitanians, appear to have maintained their independence; while in others, though the Celts were masters, the dark-skinned aboriginal people yet survived in vast numbers. It was only in the most thoroughly conquered parts of the continent that the pure-blooded Celts themselves formed the principal mass of the population. The independent dark tribes of the extreme west retained their native language, which lives on to our own time as the Basque tongue; but the vanquished and enslaved Euskarians of the central French and Spanish regions learned to speak the dialect of their Celtic lords, as they afterwards learned to speak that of their Roman conquerors.

As yet the Celts had not attempted to attack Britain, which had long since been isolated from the continent, and could now only be invaded by a fleet of boats crossing the silver streak of sea. Before they took that last step in the conquest of Western Europe, they had learned the use of bronze, from which they manufactured beautiful axes, spears, and shields, besides producing many tools for more peaceable purposes. The employment of bronze enabled the Celts to make such improvements in ship-building that they could cross the Channel to Britain, which they found inhabited only by the small dark Euskarians, who were now at a still greater disadvantage, seeing that they were only armed with stone tomahawks, while their big assailants were armed with "weapons of precision," in the shape of bronze battle-axes, lances, and spears. The consequence was that the Celts soon overran nearly the whole island, and quickly subdued the better part of it to their own dominion. In the south-eastern plains, near the Continent, they apparently settled in great numbers, so that when the Romans came they found that part of Britain mainly inhabited by a tall, fair-haired, light-skinned Aryan Celtic race. But in the west, the Celts only settled in comparatively small numbers, as lords of the soil, holding in subjection a large servile population of dark Euskarians; while in South Wales, and apparently in parts of the Scotch Highlands, the dark people remained wholly independent, as the inhabitants of those regions long afterwards did at the time of the English settlement. The South Welsh tribe of Euskarians were known as Silures, and they preserved their nationality intact down to the period of the Roman Conquest.

Now, what sort of people were the pure-blooded Celts who first came to Britain? No doubt it may be a shock to many readers to be told so, but they were undoubtedly a light-skinned, fair-haired, blue-eyed, and round-headed race—in fact, typical Aryans of the same sort as the modern Germans, and possessed of exactly those peculiarities which we ordinarily speak of as Anglo-Saxon. About this there can be no manner of mistake. Their barrows, known both by their shape and by their bronze implements, contain round skulls, quite different from the long skulls of the Euskarians; and the universal testimony of the Roman writers, whose knowledge of the Celts was obtained while they still lived in comparative purity in Gaul and South-eastern Britain, makes it quite certain that they had light hair, white skin, and blue eyes. How, then, comes it that most of us think of the Celtic type as essentially dark and black-haired? The reason is simply this. When the Celts conquered Britain, they left large numbers of Euskarians alive, in the northern and western part of the island, at least; and it is the mixed Celtic and Euskarian descendants of these people who now form the so-called Celts of the Highlands, Lancashire, North Wales, and Cornwall. Moreover, it is

certain that the Euskarians of the conquered districts soon learned to speak Celtic alone, just as the Irish are now fast learning to speak English alone; and so after a short time they became as indistinguishable from the true Celts, as Normans and Danes in England have become indistinguishable from the rest of the community. Even the Silures, who maintained their position as an independent Euskarian tribe in South Wales, seem to have acquired the use of the Celtic Welsh tongue before the date of the Roman invasion. When contrasted with the Teutonic English, all these Celtic-speaking peoples came naturally at a later period to be regarded as Celts.

Thus, at the date when Britain first became known to the civilised southern world by the Mediterranean, and before any Englishmen had yet settled in the land, its ethnical arrangement was something of this sort:—Along the southern and eastern plains, from Hampshire, by Sussex and Kent to East Anglia, Lincolnshire, and the vale of Yorkshire, there lived a light Aryan Celtic race, with more or less of subject or enslaved Euskarians—doubtless, a good deal intermixed, as negroes, mulattoes, quadroons, and whites, still are in the Southern States and the West Indies, though the light Celtic aristocracy probably kept up the purity of its own blood in the female line, as also happens in the analogous modern case. Further west and further north, among the hills of the Devonian peninsula, the West Riding, Cumberland, and the Highlands, the number of pure Celts was comparatively smaller, while the number of dark Euskarians was comparatively greater. And in Wales itself, the Silures remained as unmixed Euskarians, without a single drop of Aryan Celtic blood; while another small Euskarian principality seems also to have held out in the Athol district of Scotland. It is this compound mass of pure Celts, mixed Celt-Euskarians, and pure Euskarians, all speaking various Celtic dialects, that we ordinarily describe as Celtic, in contradistinction to the Teutonic English, who came to the country at a later date. As to Ireland, the primitive Celtic immigration there was very slight; and the mass of the population, though it acquired the Gaelic dialect of Celtic as its language, remained almost entirely Euskarian in blood up to the date of the Danish invasions, as it still remains in all except the northern and eastern coast. How far these arrangements of the various race-elements were upset by the English (or Anglo-Saxon) settlement, we shall have to inquire in our next paper.

NOTES ON ROWING.

BY AN OLD CLUB CAPTAIN.

A PROFOUND knowledge of the theory of propulsion through fluids is not essential to skilful oarsmanship, and is probably not possessed by one oarsman among ten thousand; I may go further, and note that even a perfect acquaintance with the principles of rowing may be found in company with singular inaptitude for the practical application of those principles. Shall I ever forget, for instance, how ably No. 4 in our "tub" (when I was as yet a beginner in college rowing) would discourse about the proper way of rowing, taking, if occasion suggested, a fire-shovel wherewith to illustrate the beginning, middle, and end of the stroke? but alas! in practice he could never row a mile without catching a most monstrous crab.

It is not, then, with any idea that the general run of oarsmen should study the mechanics of propulsion or the true theory of rowing either, that I pen these lines. But the subject is an interesting one, especially just now, when Oxford and Cambridge are preparing for their annual struggle; and a good oarsman is likely to be none the worse for some consideration of the *rationale* of his art.

According to the system of boat propulsion adopted in most civilised countries (but the gondolas of Venice are propelled differently) the oar is a lever of the second class, in which the fulcrum is at one end, the force is applied at the other, and the weight is somewhere between the two. The fulcrum is not fixed as in ordinary applications of the lever, for the water yields to the oar in some degree; but apart from this, the principle is precisely that of the second class of levers.

Many, however, who consider the problem of boat propulsion in this way, are perplexed by the circumstance that the oarsman himself is in the boat, and forms part of the propelled weight, while, again, the central line of the boat's breadth does not correspond with the place where the driving force is actually applied to the boat. Thus, if A B C is the oar, the rowlock at B, the place where the resistance of the water against the blade may be supposed to act at C, and the power of the oarsman applied at A, we know that in reality the weight of the boat is under A, not under B; while, again, the power P is applied within the boat itself, and whatever effect the pulling of the oarsman produces in one direction, must be exactly counterbalanced by an effect in the opposite direction. It is, in fact, the leverage alone which gives a balance of propulsive

effect. The weight of the boat is really felt at B, so far as the particular oar A C is concerned; the oarsman's strength is applied at A, and is met by an equal resistance there, but the propulsive effect at W is greater, in the same degree that the arm A C is greater than the arm B C. Thus, if an oarsman pulls at his oar with a force such as would suffice to lift one hundredweight, and if A C is 12 ft. long and A W 3 ft., then the propulsive effect at W corresponds to 112 lb. multiplied by 12 and divided by 9, or to 149⅓ lb. But the boat is not urged forward by this propulsive effect, only by the excess of this amount over the force actually exerted by the oarsman at A, so that the balance of propulsive action on the boat with its crew corresponds to a force which would raise a weight of (149⅓ − 112) lb. at the same rate as the oarsman moves his end of the oar.

It is evident that the actual leverage increases as A W is increased, supposing the oar's length to remain unchanged. But at the same rate that the leverage is increased, the velocity with which the oarsman's action tends to move the boat is diminished. Supposing C to remain at rest, and the end, A, to move with a given velocity V, the point W (at which the propulsive action is really exerted) only moves with velocity V diminished in the ratio of C W to C A. It might seem, then, that the use of outriggers diminished rather than increased the propulsive power of the oarsman—increasing his leverage, which one would have said did not need to be increased when the boat was at once made lighter and sharper—and diminishing the velocity with which his action tends to urge the boat onwards. But at the same time that the rowlock was thrown somewhat farther from the handle of the oar (not nearly so much farther as many imagine, for the old boats were wider in the beam, and their rowlocks were carried well out), the oar itself was lengthened. Owing to the diminished resistance, too, as the boat passed through the water, there is less slip of the oar through the water, which thus supplies a more perfect fulcrum. Yet the increase of velocity in light, outrigged boats is due more to the way in which they maintain their speed between the strokes than to any increased power of propulsion obtained by the oarsman. Being of smaller beam and lighter than the old racing boats, and also without keel and without laps, they maintain their velocity almost unchanged between the strokes.

And here arises a question which has been very summarily, but in my opinion very incorrectly, disposed of by many writers on rowing. It is often said that the principles of rowing are just the same now as they were in the time of the old lap-streaked inrigged racing boats. The old rules for the action of arms and body—of legs also, except in so far as the sliding seats modify their action—are therefore repeated, as if no change whatever had been rendered necessary by the changed style of boats. In other words, though it is quite certain that the new racing boats behave quite differently, though it is manifest that as they move more quickly through the water they must receive a sharper propulsion, though it is clear that with the greater leverage obtained from the use of outriggers there arises a different amount as well as a different degree of propulsion at each stroke, oarsmen (we are told) should row in the same style now as before these changes were introduced.

It would be as reasonable, I venture to say, to assert that the style of stroke suitable for a coal barge must be the best also for a wager boat. It is manifest there must be some changes, and tolerably clear what those changes should be. And as a mere matter of fact, it is seen that those rowing clubs wherein the old style of rowing is clung to, get persistently beaten, or only win

XUM

when they have a great superiority of brute force, while those who adopt a style suited to the requirements of the light racing boats as systematically win—often against great superiority of sheer strength. Again it is found that those who on their own waters are obliged to use the heavier boats, and, therefore, the old-fashioned style, are always, or almost always beaten, and that too by weaker men, when they row in the lighter boats, even when they have thoroughly mastered all such difficulties as arise merely from the relative crankiness of the canvas-covered outrigged craft.

In my next I shall show what are the differences of style which theory suggests for the lighter boats. I shall maintain the apparently paradoxical positions—(1) that the stroke must be longer, yet shorter in the water; (2) that it must be quicker, yet fewer strokes be taken per minute. I shall show how these requirements are to be secured, and I shall give evidence, which I take to be convincing, that, when they are secured, a style is acquired which utilises the oarsman's strength in the best way for contests in these cranky craft.

(To be continued.)

THE *SPECTATOR'S* COMET.

(From the *Saturday Review.*)

"*Sir Andrew Agnecheek.*—Why, this is the best fooling, when all is done."

SOME time ago—on January 28, to be particular—the mantle of Mother Shipton fell upon the *Spectator*, and she began to prophecy concerning the end of the world. Fifteen years more of life was about the exact period [*sic*] which the *Spectator* was inclined to allow to plants and animals, including man. The convulsion produced in the religious world, or rather in a part of it, was curious and pleasant to observe. The Bishop of Manchester naturally did not let such an excellent opportunity pass unpreached on, and pavid correspondents sent their views to the *Spectator*. But meanwhile Mr. Proctor has withdrawn his celebrated Menacing Comet. This phenomenon was entered, it appears, for the Scientific Sensation Stakes by the *Spectator*, "without the assent" of its owner. We really do not think that Mr. Proctor has behaved quite kindly to the *Spectator*. The *Spectator's* ambition was partly like that of the Fat Boy, as expressed in his celebrated remark to the Old Lady:—"I wants to make your flesh creep." Our contemporary was also anxious, if we may say so, to score off the Positivist and the unbeliever. But there was also manifest a very creditable desire to give Mr. Proctor and his new book "a hand." Every fellow likes "a hand," as Mr. Foker has said, with his usual artless wisdom. The *Spectator* gave Mr. Proctor "a hand," but he does not seem quite grateful. In the February number of KNOWLEDGE, a journal occupied with such topics as the "Use of Fleas," "How Spiders Fly"—[Now, dissatisfied ones, where are you, who say KNOWLEDGE is too much given to astronomy, biology, botany, and mathematics? Here is the *Saturday Review* to tell you that Mr. Mattieu Williams on the "Use of Fleas," and Professor Young on the "Flight of Spiders," out-top all other subjects]—Mr. Proctor has scratched or withdrawn that "Menacing Comet" which the *Spectator* had fondly made its own. . . . . Mr. Proctor's most significant remark in his book, we take to have been this—[here follows the reference to the outburst in the Northern Crown, in 1866.] There is a well-known sentence in an unpublished novel—"'Here is a blasted flare-up,' said the princess, whose girlish modesty had hitherto kept her silent." Mr. Proctor had been explaining that what the bashful princess called a "blasted flare-up" had occurred in the new star of the Northern Crown in 1866. And he had said that the "flare-up" was probably caused by the motion of the comet followed by a meteoric train. . . . What conclusion could the *Spectator* draw from all this?—and from Mr. Proctor's refusal to say "that there is *absolute* danger in the case of our own sun when the comet of 1843 shall be absorbed by him?" "What conclusion could be drawn, except that Mr. Proctor thought the comet a serious matter and the odds against the destruction of life in the world very short odds indeed? And now Mr. Proctor distinctly declares (and we are glad to have his assurance), that "there is not the slightest reason to fear that the comet will do any harm to the solar system when finally absorbed." Mr. Proctor, in KNOWLEDGE, says that he has elsewhere shown that "all comets of the destructive sort"—all "rogue comets," if we may adopt a term from the Jumbo controversy—have long since been eliminated from the solar system. Mr. Proctor gives pictures of comets of the rogue and peaceable varieties. The menacing comet, or, as we may now call it, the domesticated comet, is a very slim and scanty one. Then we have a likeness of a comet which might have been dangerous if it had gone the wrong way. Then we have a portrait of a comet . . . . with a gigantic eye in its head, and a bushy and furious tail. However, that is the look-out of some other solar system, and not ours. Our solar system, we again repeat [*sic*] is all right . . . . unless a comet like Donati's "gets its head," bolts, and comes straight for the sun. As for the comet of 1880, in future it will be known as "the *Spectator's*," or perhaps "the Bishop's comet."

NEWTON'S VIEWS AS TO A MENACING COMET,

AND AS TO THE HEAT OF THE SUN'S INTERIOR.

AMONGST the MSS. referring to Sir Isaac Newton, in the possession of the Earl of Portsmouth, is a paper in the handwriting of Mr. Conduitt, who married Catherine Barton, Newton's favourite niece; it purports to be notes of a conversation held with Sir Isaac about three years before his death, and contains a conjecture with respect to the ultimate fate of the comet of 1680, which bears a striking resemblance to the prediction which has recently been attributed to Mr. Proctor.

Mr. Conduitt, who was Newton's successor at the Mint, had the intention of writing a life of Sir Isaac, and commenced making notes for the purpose, but he had but little literary talent, and the project was soon abandoned. The notes, however, have been preserved, and though they have never been printed at length, the paper from which the following extract is taken is given in Turnor's "History of Grantham," a rather rare book, which is not in the Library of the Astronomical Society. Mr. Conduitt says—

"I was on Sunday night, the 7th of March, 1724-5, at Kensington with Sir Isaac Newton in his lodgings, just after he was come out of a fit of the gout, which he had had in both his feet, for the first time, in the eighty-third year of his age. He was better after it, and his head clearer, and memory stronger, than I had known them for some time. He then repeated to me, by way of discourse, very distinctly, though rather in answer to my queries, than in one continued narration, what he had often hinted to me before, viz., that it was his conjecture, he would affirm nothing," . . . . . [I omit a paragraph with respect to the planets] that "a comet after certain revolutions by coming nearer and nearer to the sun would have all its volatile parts condensed, and become a matter fit to recruit and replenish the sun (which must waste by the constant heat and light it emitted), as a faggot would this fire, if put into it (we were sitting by a wood fire) and that that would probably be the effect of the comet of 1680 sooner or later, for by the observations made upon it, it appeared, before it came near the sun, with a tail only two or three degrees long, but by the heat it contracted in going so near the sun, it seemed to have a tail of thirty or forty degrees, when it went from it; that he could not say when this comet would drop into the sun; it might, perhaps, have five or six revolutions more first; but whenever it did, it would so much increase the heat of the sun, that this earth would be burnt, and no animals in it could live. That he took the three phenomena seen by Hipparchus, Tycho Brahe, and Kepler's disciples, to have been of this kind, for he could not otherwise account for an extraordinary light as those were, appearing all at once amongst the fixed stars (all which he took to be suns enlightening other planets, as our sun does ours) as big as Mercury, or Venus, seems to us; and gradually diminishing for sixteen months and then sinking into nothing."

In the light of our present knowledge with respect to the mass of comets—and the probable origin of the sun's heat, such a theory is, of course, untenable, but the speculation is of interest, as it serves to show that Newton must have suspected the existence of a resisting medium in the neighbourhood of the sun. If he had not, as is, possible, satisfied himself from the observations of the total eclipse of May, 1715, that the solar corona then observed* had a real existence, and was not merely an optical illusion.

Long before this eclipse, it is evident that Newton suspected an atmosphere outside the photosphere. In the "Principia," Book III. (I quote from Davis's translation, published in 1803, at Vol. II., p. 307), he says: "The comet which appeared in the year 1680 was in its perihelion less distant from the sun than by a sixth part of the sun's diameter, and because of its extreme velocity in that

* Several drawings were made of the corona observed during this eclipse—woodcuts from two of them are given in Edleston's correspondence of Cotes and Newton. From the account of the eclipse in the *Memoirs de l'Académie*, it is evident that several drawings were made by French observers. Newton himself no doubt observed this eclipse, though he does not appear to have done so in company with his friend Halley and other members of the Royal Society, who observed it from the roof of the Society's House in Crane-court, out of Fleet-street. From a letter of Cotes, it is evident that Newton was not at Cambridge at the time of the eclipse.

proximity to the sun, and some density of the sun's atmosphere, it must have suffered some resistance and retardation; and therefore, being attracted something nearer to the sun in every revolution, will at last fall down upon the body of the sun."

Another fact with respect to Sir Isaac Newton's correct judgment as to the internal heat of the sun is worthy of being mentioned, especially as a contrary theory with regard to a cool dark body within the photosphere survived down to the time of Sir John Herschel. In a letter dated 16th April, 1681, Sir Isaac Newton says, "Now though the inward part of the sun were an earthy gross substance, yet, if the liquid shining substance, which Mr. Flamsteed supposes to swim upon it, be then hot, it will heat the matter within as certainly as melted lead would heat an iron bullet immersed in it. Nor is it material whether the liquid matter on the sun be of any considerable thickness. An iron bullet would heat as fast in a quart as in an ocean of melted lead, this difference only excepted, that the bullet would cool a small quantity of lead more than a great one. If, then, the liquid matter swimming on the sun be but so thick as not to be cooled by the central parts (as it must be), it will certainly heat the central parts, for it imparts heat to the contiguous matter as fast as if it were thicker, and keeps all cool environing mediums (the instrument of cooling things) from coming near the central parts to cool them. By which means the central parts must become so hot, as if the hot fluid matter surrounding it equalled the whole vortex. The whole body of the sun, therefore, must be red hot," &c.

A. C. RANYARD.

INTELLIGENCE IN CATS.

CORRESPONDENTS of KNOWLEDGE, in treating of cats, do not seem to have remarked some acts of intelligence which may be observed daily in the streets of London. At the cry of the cat's-meat man all the cats are in commotion, but all are not excited by the cry of the same man. A dozen men may walk up and down a street with the tempting morsels, calling "meat, meat!" but only at those houses which they are accustomed to serve will the cats be roused by the call. No sooner does the proper man arrive in a street than every cat he is accustomed to serve rushes frantically to the door, or, if allowed, into the street, running mewing towards him, rubbing against his legs, or sometimes sitting in a begging attitude before him, but never, as far as I have observed, attempting to steal from the open basket.

One day I noticed a cat whose man had either forgotten her portion or had been unable to make her mistress hear, and so had passed on. The cat, however, insisted upon being attended to; she ran after him, mewing piteously, and when at last she made him understand, she ran back to the house before him, where by that time the mistress was ready to take the delicacy so much prized by all London cats, however well fed. I have often watched this act of discrimination in our own cat. Tom would sit quietly dozing whilst man after man went by with the familiar cry of "Meat, meat," but presently he would jump up, rush to the window, and remain in a state of great excitement, and soon after a distant cry of "Meat" might be heard, and we knew that Tom had recognised his own man long before we had heard him. As the cry drew nearer, Tom's excitement increased, and he would almost fly to the door. A singular fact remains to be told. On Saturdays the man would leave two portions, as he did not go his rounds on Sundays. These were often thrown into the area, to which Tom had access. He would always greedily devour the one portion, but never touch the other, although they lay side by side. This cat would also open the latch of the kitchen-door, as observed by several of your correspondents, and would also open the shutters in the drawing-room (closed but not fastened), in order to look out of window. I have, however, been told of a cat who would open not only a latch, but an ordinary drawing-room door, rather loose, by taking the round knob between her paws and twisting it round and round till it opened.

The fact of cats distinguishing between one meat man and another seems to me to disprove the oft-repeated assertion that cats attach themselves only to places, and not to persons; for here we see them able to pick out a certain man by his voice alone, even at a great distance.

A. W. BUCKLAND.

WOOD-GAS.

IF I may be permitted to do so, I should like to say a few words in reply to Lewis Arundel's "elucidation" of my abstract on "*Wood-gas*," *not* "Carbon Monoxide," as he construes my meaning.

If, on line 12 of the abstract, page 246, the word *can* between the words "*we obtain*" had been inserted, Lewis Arundel might have been saved the trouble of elucidating my description (or—more strictly speaking—my informing the readers of KNOWLEDGE that there was such a thing) of "Wood-gas." I am sorry I did not make my meaning clearer.

The "dangerous, obnoxious, and otherwise objectionable products" referred to are N : S : H_2S : CS_2 and tar. N in the form of ammonia, which is virtually absent. Little sulphur can exist in any form, while there is but a trace of sulphuretted hydrogen or of carbon bisulphide, and there is not a trace of tar.

I did not previously enumerate these for fear of taking up, needlessly, too much space in your paper.

Lewis Arundel evidently mistakes the acid taste (or, perhaps, the tingling in the nose, when inhaled through that organ), of CO_2 for an odour; that is to say, if he is dealing with CO_2 in a *pure* state when he perceives the odour about which he is so certain. He might as well say that hydrogen had an odour, because he himself had never obtained it odourless. I cannot think he could be in earnest when he said (*re* CO_2), "whatever our text-books may *copy* from one another." Is it likely that the leading chemists of the day, in writing their text-books, simply "copy from one another" without being themselves thoroughly conversant with the peculiar properties of the subject under their notice?

What would Lewis Arundel say to the man who told him that the snow they were looking at was green, because he (the man) was unaware that he was looking at the snow through green glasses? This is not a bit more ridiculous than to say that chemists "copy from one another," *re* the odour of CO_2.

The blue flame of CO is hardly observable over a *sluggish* fire; on the contrary, it is seen on the top of a "glowing" fire, with a background of red-hot bricks. Lewis Arundel would see how the CO is formed by referring to my text-book.

He now asks, "How, then, can it confer on 'an inferior coal-gas' a 'great candle-power'?" This, I own, is not quite clear, my mistake being that, in trying to be concise, I was too concise—when I say that the coal and wood-gas are mixed with *naphtha vapour*, Lewis Arundel will see how "the great candle-power" is conferred. I may say, before concluding, that "after the first charge of wood has been carbonised, the charcoal, instead of being drawn, as with coke, is pushed back into the retort," &c., &c. This is repeated with the second charge, and so on.

Anyone who has to make the gas, carbonic oxide, will see the caution given in all text-books, *i.e.*, its very poisonous property.

F. C. S.

FOR NEXT WEEK.

NEXT week, an interesting paper by Dr. Ball, on the Future of the Earth and Moon, will appear, and one by Dr. Carpenter, on Dr. Siemens' recent communication to the Royal Society, respecting a possible use of the seemingly waste energies of the sun. In response to a number of queries respecting the motions of planets with respect to the earth, and the explanation of the advance, retrogradation, and stationary points of the planets, I have determined the distance and bearing of the planet Mars from the earth at intervals of ten days from the opposition of 1875 to that of 1892, and, having set them down in a chart, have carried through them the various convolutions representing the path of Mars during those seventeen years. The result (which is rather curious, and represents much more labour than many would perhaps suppose) will be presented next week in a two-page chart.

RICHARD A. PROCTOR.

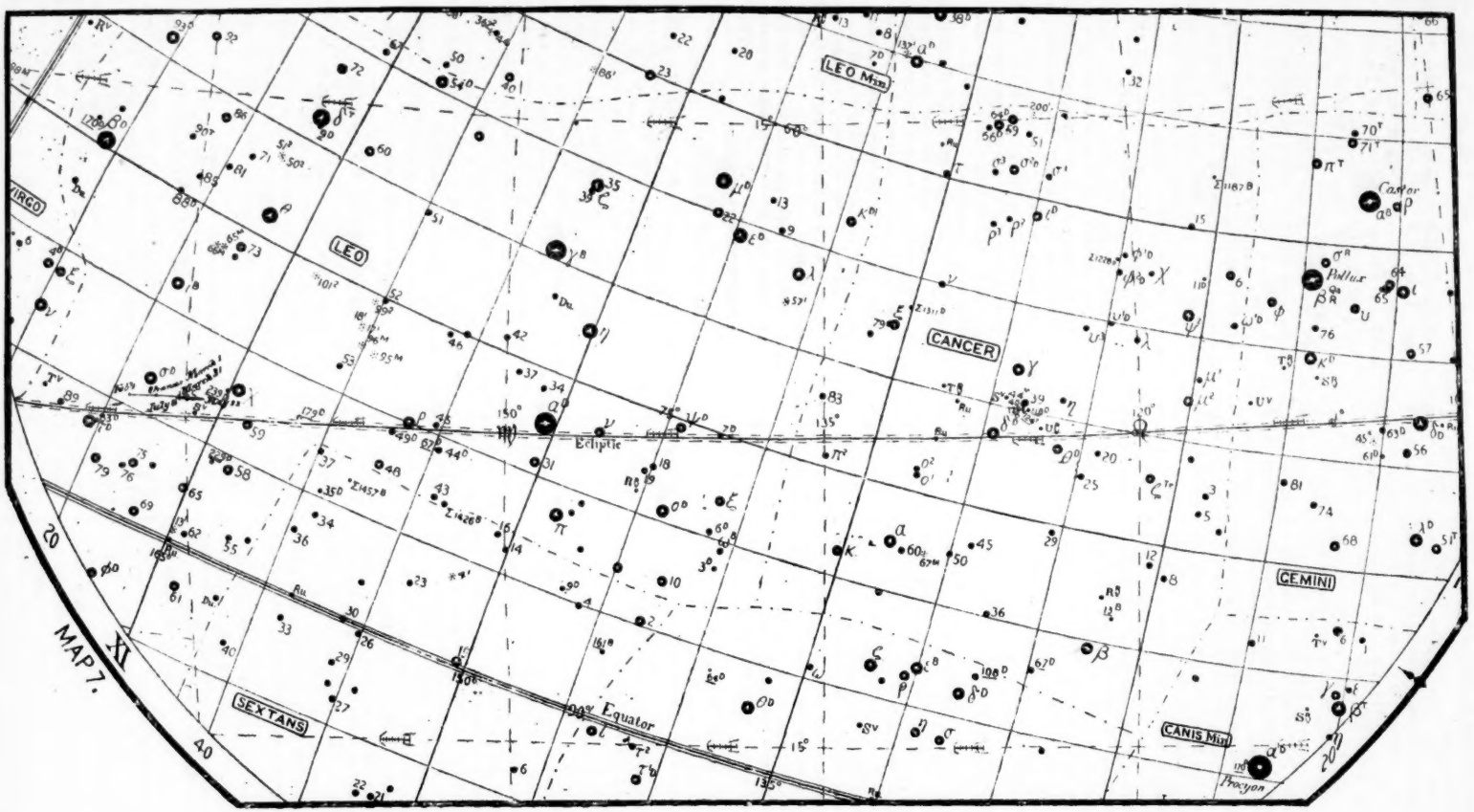
METEOROLOGICAL REPORTS.

I PROPOSE from and after the vernal equinox, at latest, to publish weekly reports of the weather, based on the daily records obligingly supplied from the Meteorological Office. As yet, the plan on which these reports will be given has not been decided upon, nor can we at present assign the space we can afford to give to the subject.

OMISSIONS AND CORRECTIONS.

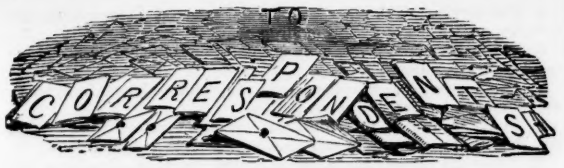
WE omitted to notice in the foot-note relating to the book by Dr. De Luys, on the Brain, that it forms one of the International Scientific Series, published by Messrs. Kegan Paul & Co., and that its price is 5s. The treatise on the Sun, by Professor Young, has been included (since our review appeared) in Messrs. Kegan Paul & Co.'s announcements as belonging to the same series, at the same price. For the *Nineteenth Century*, in the "Answers to Correspondents" for last week, p. 393, 1st col., line 17, read the *Century* (which, we believe, was what was originally written).

XUM



ZODIACAL MAP.

WE give this week our second Zodiacal map, showing, in enlarged scale, the part of the Zodiacal region best suited for observation during this month and the next. The path of Mars during March is just outside this map in Gemini, and for it the reader is referred to our first Zodiacal map in Part III. (Number 11). The path of Uranus during its present opposition passage is shown. No other planets occupy this portion of the Zodiac at present.



Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

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*** *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

VEGETARIANISM.

[314]—In the last number of your interesting journal, I observe that F.R.A.S. desires a question answered relating to myself (whom he has referred to by name as a reputed advocate of vegetarianism); to wit—Am I also a vegetarian by practice?

Most unwilling as I am to obtrude unnecessarily any opinions of mine, still more to speak publicly of my personal habits, I cannot permit any such misapprehension respecting them to pass current, as the foregoing inquiry implies.

First, as to my opinions respecting an exclusively vegetarian diet for man: these have been already so distinctly stated in a little work, entitled "Food and Feeding," which has had a wide circulation, that I am astonished to learn that any one can describe me as a vegetarian; supposing that term to denote one who desires to restrict himself or others to food composed of cereals and vegetables. If the term does not mean that, it is wrongly used; for no consumer of eggs, milk, cheese, or butter has the slightest claim to the title—a remark which ought to be quite unnecessary, but is not so.

To this little book permit me to make the briefest possible reference. At page 21, I give my reasons why man should be regarded as an "omnivorous animal," and why there is no *à priori* ground for "limiting his diet to products of either kingdom exclusively." I go on to show that the relative amounts of animal and vegetable constituents in diet vary according to the climates, hot or cold, in which man lives. But at page 27, I express a belief that Englishmen generally eat more animal food than is desirable for health; that meat, fish, eggs, and milk are generally essential for those who perform much mechanical labour; while fish and lighter flesh are better adapted to brain-workers, &c.

To this I need add nothing here.

Secondly, as to practice. It is quite true that I think it worth while to devote some attention to the growth of vegetables, and to furnish from my garden an ample supply of fresh produce, such as I can obtain nowhere else. But although thus enabled to enjoy the luxury of fresh green food in variety daily throughout the whole of our long winter, I am glad to consume fish and flesh of all kinds in addition, although, probably, in smaller proportion than most people believe to be necessary. Let this suffice in reply.

But now, let me further add that nothing can in my opinion be more unwise than to debar ourselves from the right to utilise food of any and every kind. Human stomachs differ so widely—why not as widely as facial physiognomy?—that an unlimited dietary is really the last thing a man who has so much to do with digestive laws and tastes (my own and others) can afford to dispense with. For diet, to be wholesome, should be varied for all; but it must also differ for each, in relation to his habits, sedentary or active, whether he is occupied with muscle work or brain work, &c. Diet, too, must differ, not merely for the young and old, but has to be changed for different epochs of life, as any one who has observed the subject closely, or himself has lived a good many years, must of necessity have learned. How much might be said on this head. And, in presence of various ailments of the body, how complex becomes the subject sometimes! How little all this seems to be taken into account by people who would limit our resources in the matter of food. It follows from all this, also, that I should be the last to deny that some constitutions thrive better on vegetables and cereals alone, than by admixture with animal food, since this is one of the many variations which nature produces. And I think I might add that an exclusively animal diet would probably (in our temperate zone, observe) be still more rarely found the best for any man, although it might, perhaps, be so for a very few.

So many persons hastily conclude that what is best for them is necessarily best for all the rest. There is no greater error, and none of us can be too watchful against its influence.

35, Wimpole-street. HENRY THOMPSON.

[315]—I am much surprised that "A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY" (letter 215, page 362) should consider that the idiosyncrasies, or some of the idiosyncrasies, of the Irish are due to their feeding on potatoes. This seems to me a very absurd statement to make, but I can put no other construction on his words. I am sorry that he is not more definite, and does not say exactly in what way this diet can influence Irish character. If this statement were worth consideration, I should ask him to show that the Irish character has changed since the introduction of this tuber. I believe that whether you feed a man on potatoes or on beef, if an Irishman he remains an Irishman, and if an Englishman he has still the characteristics of an Englishman.

Supposing the charges made against potatoes and rice were substantiated, it would not militate against vegetarianism. I consider that they are very unsuitable articles of food to live almost entirely on, and one reason is this: We require a certain amount of nitrogenous food (of which albumen may be taken as the type). Dr. Lyon Playfair prescribes 18 parts of carbo-hydrates to 4 of nitrogenous and 1 of fatty matter. This, in percentages, is 78, 17½, 4½ respectively. Now, I do not think fat necessary, as the carbo-hydrates can and are converted into it in the body. This makes 82½ of carbo-hydrates and fat, to 17½ of nitrogenous matter. Neglecting the water, indigestible fibre, and mineral matter, the proportion of these two in a few foods are—Potato, 88 to 12; Patna rice, 92 to 8; fine Scotch oatmeal, 82 to 18; wheat flour, 86 to 14; lentils and haricots, 70 to 30. It will be seen that potatoes and rice are deficient in nitrogen, so that, to supply a sufficiency of this, a superabundancy of other matter has to be taken.

A FELLOW OF THE CHEMICAL SOCIETY.

INTELLIGENCE IN THE DOG.

[316]—I have a very fine, large dog—a cross between a Newfoundland and a retriever—that really at times astonishes me by his intelligence. I have also—to make the story complete—a Skye terrier, that is not over kind with his mate. Last summer, during hay-time, a neighbour came to assist me one day in carting hay, bringing with him a sheep-dog. This dog, whenever the cart stopped in gathering up the hay, went and lay down under it. The Skye terrier thought it might do the same, but this was too great an intrusion to be endured by the sheep-dog, so a furious fight ensued under the cart, the sheep-dog eventually dragging the Skye into the open field. Standing about fifty yards from the cart, and thinking the little rascal about to be badly used, I began to run for the scene of danger. The large dog, who was near me, saw the danger in which his mate was involved as well as I did, bounded off at full speed, up with his right fore leg, and hit the cur such a stroke over the side as sent him rolling twice over. The Skye flew to me, evidently thankful to his powerful friend. The cur retired beneath the cart.

The same dog, only a few days ago, showed to my mind a wonderful amount of sense. Two curs were fighting furiously, the blood flying in all directions. Two men were trying to separate them, each seizing one by the tail. No sooner, however, were they let go, than they at it again. This dog was at the distance of a hundred yards at least, with a hedge between him and the combatants, but so placed that he could see them. He looked for a

XUM

while, but seeing the effort of the men ineffectual, he bounded off at full speed, and by means of his powerful forelegs he sent one dog flying to the left and the other to the right in wild confusion. There he stood, with the dogs at a distance, as much as to say—Dare to come again, either of you! No further attempt at fighting occurred. He is a very powerful dog, about 7 stone weight, and is excellent in clearing the road of cattle and sheep before the trap. He rarely uses his teeth, unless attacked, his forelegs being his chief weapons. THOMAS FAWCETT.

[317]—Let me add to the instance given on page 269 that a dog living at a boarding-house, at 51, Sussex-gardens (where I used to live), laughs in the most human fashion, showing all his teeth. This, with him, is a greater sign of pleasure than wagging his tail.

P.S.—Is it not worth your while to correct the impression that your correspondents have, that Darwin is to be accredited with being the first biological evolutionist? Cannot you put Herbert Spencer's position in this regard right? J. H. B.

[318]—I have a little black dog, which, if put out of the room, the door shut, and a ball, or even a stone, hidden, will, when let in, make a thorough search for it, first making a circuit of the room, smelling and looking the while, until he finds it, which he accomplishes in a very short space of time, being as "proud as Punch" when he has recovered it. The second time he comes in he will immediately go to the place where the stone was last hidden. Will he not reason that as the stone was there last time it will be there now also? The other day I hid it in my pocket, which he found in due time. Two days after I hid it again; as soon as he was let in he put his nose right into my pocket. One can imagine him finding a ball by its smell, but how is it that he can find a stone sooner than a human being could? GERTRUDE A. FRYER.

ELECTRICAL IMAGES (228).

[319]—We have

$$P_b\,[q^2_{ab}-q_{aa}\,q_{bb}]=E_a\,q_{ab}-E_b\,q_{aa} \qquad \text{(i.)}$$

and expanding in powers of b, and neglecting b^3

$$q^2_{ab}=\frac{a^2b^2}{c^2}\;;\quad q_{aa}\,q_{bb}=ab+\frac{a^2b^2}{c^2}+\frac{a^2b^2}{c^2-a^2}$$

$$\therefore\; q^2_{ab}-q_{aa}\,q_{bb}=-\left[ab+\frac{a^2b^2}{c^2-a^2}\right] \qquad \text{(ii.)}$$

and again

$$E_a\,q_{ab}-E_b\,q_{aa}=E_a\left[-\frac{ab}{c}-\frac{a^2b^2}{c(c^2-a^2)}\right]-E_b\left[a+\frac{a^2b}{c^2}\right] \qquad \text{(iii.)}$$

Hence

$$P_b=\frac{E_a\left[\frac{a}{c}b+\frac{b^2a^2}{c(c^2-a^2)}\right]+E_b\left[a+\frac{a^2b}{c^2}\right]}{ab+\frac{a^2b^2}{c^2-a^2}}$$

$$=\frac{E_a}{c}+\frac{E_b}{b}\left[\frac{a+\frac{a^2b}{c^2}}{a+\frac{a^2b}{c^2-a^2}}\right]$$

$$=\frac{E_a}{c}+\frac{E_b}{b}\left[1+\frac{ab}{c^2}-\frac{ab}{c^2-a^2}\right]$$

$$=\frac{E_a}{c}+E_b\left[\frac{1}{b}-\frac{a^3}{c^2(c^2-a^2)}\right] \qquad \text{(iv.)}$$

which is the required result. R. STAVELEY.

DARWINISM.

[320]—If the opponents of Darwin's theory will consider with care the following, from the pen of Prof. Fiske, of Harvard College, they may, perhaps, be convinced. He says, in "Cosmic Philosophy," that the Darwinian theory, when analysed, resolves itself into eleven propositions, nine of which are demonstrated truths; the tenth a corollary to the nine predecessors; the eleventh a perfectly legitimate postulate.

1. More organisms perish than survive.
2. No two individuals are exactly alike.
3. Individual peculiarities are transmissible.
4. Those individuals whose peculiarities bring them into close adaptation with the environment, survive and transmit their offspring.
5. The survival of the fittest thus tends to maintain an equilibrium between organisms and their environment.
6. The environment of every group of organisms is steadily, though slowly, changing.
7. Every group of organisms must, therefore, change in average character, under penalty of extinction.
8. Changes due to individual variation are complicated by the law that changes set up in one part initiate changes in another.
9. These are further complicated by the law that structures are nourished in proportion to their use.
10. The corollary follows:—That the changes thus set up and complicated must alter the specific character of any group of organisms.
11. [The only postulate] Let it be granted that, since the first appearance of life, time enough has elapsed to produce all the variation of species now seen. C. T. B.

ARRANGED SQUARES.

[321]—Your correspondent, E. V. R. (letter 231, page 273), says:—"I believe these squares may be arranged by placing the diagonal numbers in what I may call their *natural* squares in the first instance, and working up to them; but I have only *succeeded* with the square of 4."

As I have succeeded with the squares of 5 and 6, I send my solutions, which, I believe, will be found correct.

A number in any place, added to the number in the corresponding place on the opposite side of the square, will make 26.

1	20	16	23	**5**
15	**7**	12	**9**	22
24	18	**13**	8	2
4	**17**	14	**19**	11
21	3	10	6	**25**

The perpendicular, horizontal, and diagonal lines will make 65.

A number in any place, added to the number in the corresponding place on the opposite side of the square, will make 37.

1	20	33	34	17	**6**
32	**8**	30	7	**11**	23
24	35	**15**	**16**	2	19
18	10	**21**	**22**	27	13
5	**26**	9	28	**29**	14
31	12	3	4	25	**36**

The perpendicular, horizontal, and diagonal lines will make 111.

EMMA C. HOOTON.

GHOSTS.

[322]—Perhaps some of your readers may be able to supply details as to the War Office ghost. All I now remember is the fact which gave the case its name and notoriety, viz., that as there was a discrepancy between the date of the apparition and that of the official return of the death, further inquiry was made, at the instance of the relatives, by the War Office authorities, with the result that the actual date of death coincided with the time of the apparition, and that the former official return was incorrect.

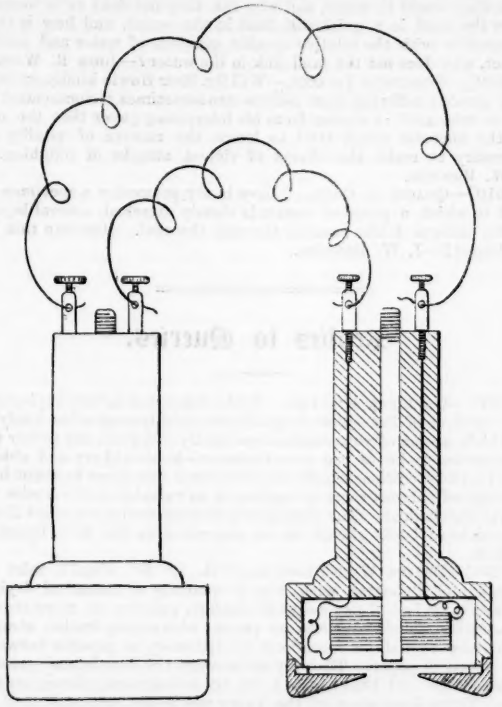
I do not see that instances of mistaken identity, and so on, have much bearing on a case like this. Of course, it is the strong and not the weak cases with which science has to do. T. D.

TELEPHONE.

[323]—There is no great skill, or extensive apparatus required, in the construction of the ordinary Bell telephone; and I can promise Mr. Mortimer (Query 222, p. 303) complete success, if he will carefully work out the following instructions. The telephone consists of a small permanent magnet, a coil of fine silk-covered copper wire, a diaphragm of very thin iron, and a case for containing and fixing the whole.

XUM

The magnets may be from 4 to 6 in. long, and ⅜ to ½ in. in diameter, and will cost per pair from 2s. to 2s. 6d. The coils of fine wire, the same as magnets, and the iron, which is known as ferrotype, about 4d. per pair. The cases, if bought ready turned, will cost about 6s. the pair. Now, as this is the most expensive item, I will show that with a little ingenuity this may be overcome at about one-tenth the above price. At any chemist's procure a couple of empty violet-powder boxes, at the outside fourpence, and see that the lids fit tightly. Then, in the centre of the lid, bore a hole ⅜ of an inch in diameter, then pare away carefully all around the hole until you get a shallow, funnel-shaped mouthpiece. Finish off with a piece of glass-paper. Now cut a ring of cardboard, ½ of an inch wide, that will just fit inside the lid of your box. Take one of the pieces of iron, and, with a pair of scissors, cut a disc that will also fit inside the lid of box, drop in the ring of cardboard, and next the iron disc, and see that the cardboard is thick enough to prevent the disc from touching the lid anywhere but at the edges, the middle being free to vibrate. In the bottom of the box, bore a hole just large enough to take the magnet easily. You now want some sort of a support for the rest of the magnet, and also to form a handle. This can be formed from a round piece of wood about 3 in. long and 1 in. in diameter, with a hole through the



centre lengthwise to take the magnet. This must be glued to the bottom of the box, the holes corresponding; or, should you find any difficulty in shaping the stem, take a large cotton reel, the end of which will form a sort of flange, and will be better for glueing. You are now ready for putting the various parts together. Push the magnet up through the reel until the end projects within the box, and stands almost level with the edges of the box without the lid; slip the bobbin of wire over end of magnet in box, and fix it so that about $\frac{1}{16}$ of an inch of the magnet is above the reel or bobbin. Pass the two ends of the fine wire through bottom of box, and solder to two brass binding screws fixed somewhere on the case, the proper place being at the end of them. Put the lid (with diaphragm and ring in place) on the box. The diaphragm will now be firmly pinched between the lid and box, and should be just clear of the end of magnet. Your telephone is now complete, and can be polished, or varnished with a spirit varnish, and when dry is ready for use. To connect, simply take a wire from each binding-screw on one telephone, and connect to each binding-screw on the other, and the instruments will speak. But beware of a very common error, and don't try to speak into one and hear your own voice in the other. This is impossible. Have at least a dozen yards in each screw, and leave one instrument with a friend while you take the other into another room. Speak distinctly into the mouthpiece, and when hearing keep the mouthpiece tight up to your ear. For connecting, use cotton-covered copper about No. 20.

Should the voice seem very distant, move the magnet nearer the diaphragm. If no voice is heard, you will most likely find the magnet sticking to the diaphragm, the proper distance being about the thickness of a piece of stout paper.

As now used and sent out by the different telephone companies, the telephone is never used both as a receiver and transmitter. The transmitting instrument being a microphone of peculiar construction, this adds greatly to the utility of the instrument, as you are able to carry on conversation without removing the receiving telephone from your ear, all the speaking being done at the microphone.

I shall be most happy to send a description of the microphone, both simple and as a transmitter, as made by G. E. V.

PROBABILITIES IN CARD DRAWING.

[324]—Chas. A. Edes writes, with reference to the problem in probabilities at p. 301, that the cards are not shuffled between the cuts, so that the cutter, if he fails the first time, has a rather better chance next time, because he will not cut in the same place again, and similarly for the third trial. To solve the problem in this form, treat it as though the card cut were removed from the pack at each failure. The solution would then run thus:—

The chance that a winning card will not be cut the first time is $\frac{10}{13}$. In that case there remain 51 cards, of which 12 are winning ones. Thus, the chance that there will be a second trial, and that a winning card will then not be drawn, is $\frac{10}{13} \times \frac{39}{51}$. There then remain 50 cards, of which 12 are winning ones; and the chance that there will be a third trial, and yet again a winning card not be drawn, is—

$$\frac{10}{13} \times \frac{39}{51} \times \frac{38}{50} = \frac{38}{85}$$

The odds in favour of drawing one of the winning cards at one of the three trials are therefore 47 to 38, instead of the smaller odds 1,197 to 1,000. It is obvious, of course, that in the closing sentences of the examination of the problem at page 301, I should have written "in favour of *A*" instead of "against *A*." EDITOR.

CENTRIFUGAL FORCE AND AN OPTICAL ILLUSION.

[325].—When a piece of twine is attached at one end to a ring, and the other end, held between finger and thumb, twisted and untwisted rapidly, the ring, when it has acquired sufficient velocity, spins horizontally in either direction, according to the twist given to the twine, and the eye is deceived by the appearance of a second string attached to the opposite side of the ring, the ring and two strings forming a perfect cone, whose apex touches the finger and thumb. A gutta-percha ring, about 2 in. in diameter, shows this simple experiment to advantage. E. C.

[This is a very pretty illusion. If the weight of ring is small, we see a nodal chord, generally a third of the way up. The apparent cone is not perfect. It is easily seen why the cone seems bounded by two distinct strings.—ED.]

SIGN OF "BEAR AND RAGGED STAFF."—Shakespeare refers to this as the crest of Warwick, "the king-maker," in 2 Henry VI., act v., sc. 1, where Warwick is made to say:

"Now, by my father's badge, old Nevil's crest,
The rampant bear chain'd to the ragged staff."

—W. MIDDLETON BUTTERS.

INTELLIGENCE IN ANIMALS.—About twelve years since, when I was living at Richmond, I had a fine black retriever. She was an exceedingly docile, tractable animal with people, but always shunned animals, even those of her own species. One day, walking with her through the streets of the town, she was persecuted by a large, coarse, ill-bred, bull-dog; she snapped at him several times. Presently I turned down to the water-side; to my surprise, here she began playing with the dog, and soon after she gambolled about in shallow water with him, occasionally swimming. All at once he got out of his depth, and, to my astonishment and dismay, she got him by the nape of the neck and kept his head under water. I shouted "Down, Prin, down!" but of this she took no notice, until she had almost suffocated him; then she let him go, and he dragged himself out of the water, and slunk away with his tail between his legs, and went his way, a sadder, and I hope a wiser, dog.—J. BROWNING.

XUM

Queries.

[287]—Ferrier's Influenza Powder.—Can anyone give me Dr. Ferrier's prescription for influenza, made up of bismuth, acacia powder, and morphia, to be taken in the form of snuff?—Afflicted Subscriber.

[288]—Plants in Bedrooms.—Are growing plants unhealthy in the bedroom? If so, should be glad to know the reason.—J. C. L.

[289]—The Burial of Moses.—Where can I obtain the poem entitled "The Burial of Moses," or where does it occur?—J. C. L.

[290]—Tertiary Fossils.—Can any of your readers explain how it is pyritous fossils from tertiary beds are so apt to fall to pieces, while those from secondary strata remain unaffected by exposure to the air? I have examples of ammonites, &c., in my cabinet that have been there for years, and, although quite "brassy" in appearance from pyrites, yet remain entirely unchanged.—W. D. C.

[291]—Razor.—Why does a razor cut better after it has been dipped in hot water? This query answered on philosophical principles will oblige.—R. Joseph Houlton.

[292]—Silver.—How can I melt the residue of old silver baths so as to obtain pure silver? Can it be done in any way without using a crucible? How much can I obtain a crucible for?—F. A. B.

[293]—Smelling Salts.—Is the use of smelling salts beneficial or otherwise? Why are they resorted to profusely by women, but not by men?—F. M.

[294].—Exceptional Seasons.—How is the exceptionally severe winter of 1880-81, and the contrast in the exceptionally mild winter of 1881-82, to be accounted for?—F. M.

[295]—Apparent Paradox in Probabilities.—A bag contains an indefinitely great number of marked tickets, the nature of the marks being unknown. One hundred tickets are drawn. These all bear letters of the alphabet, viz., 50 bear A, 30 B, and 20 C. Then (*v.* Lubbock and Drinkwater Bethune's "Probability," p. 27), the chance that the next ticket drawn will bear a letter of the alphabet is $\frac{101}{102}$; but the chance that it will bear A or B or C is $\frac{103}{104}$. That is to say, the chance of the next mark being one of the 3 letters A, B, C, is greater than the chance of its being one of the 26 letters A to Z. Where is the fallacy in the reasoning?—Gradatim.—[In the former case we have the probability that an event will be either of one kind shown to be prevalent in a certain degree, or will not be of that one kind. In the latter we have the probability that it will be one of three kinds shown to be prevalent in certain degrees, or will not be of those three. The evidence for three distinct kinds of marking gives stronger reason to believe that the next will be one of those, than the evidence of one sort of marking gives in favour of the next belonging to that one kind. The paradox seems to arise from this, that in calculating the chance of the next being a letter, we do not take into account the evidence tending to show that there are three prevalent letters in the bag. See also De Morgan on "Probabilities."—Ed.]

[296]—Photographic Studio.—I wish to construct a reasonably inexpensive glass-house, and shall feel obliged by a few hints as to (a) dimensions, (b) material for walls and roof, and (c) arrangement of light, having regard to the fact that the studio will have to join the back of my house, which has a S.W. aspect. A reference to any work on the subject will also oblige.—Amateur.

[297]—Lime-light Apparatus.—Where could I best obtain one cheap, suitable for illustrating lectures to workmen with, and what would be about the probable cost of the instrument complete?—W. R. L.

[298]—Our Ancestors.—We are told that the palæolithic man who inhabited Britain was black in Mr. Grant Allen's interesting article. How is this known to be so? There is no evidence offered in the article, and I should be glad to know how this can be proved.—Homo Palæolithicus.

[299]—Magic Lantern.—Is it possible to make a magic lantern in wood suitable for an ordinary room? If so, could you kindly give directions, or mention some book (with price) on the subject?—Amateur Carpenter.

[300]—Does the learning by heart of prose or poetry improve or impair the faculties?—A. C.

[301]—Magic Lantern.—I want to make a lantern for scientific illustration. Can any reader tell me the size, focal length, and distance apart of the glasses to be used in making one to take photographic slides?—F. D. H.

[302]—Vegetarianism.—Can you tell me of a good book hereon? I know not what vegetables to eat, or how to cook them.—John Alex. Ollard.

[303]—Destruction of Sodom and Gomorrah.—A clergyman preaching on this subject stated that it had been almost conclusively proved, on scientific grounds, that these towns were destroyed by a shower of meteors on July 31, 1898 B.C. Is there any proof of this; and, if so, what is the proof?—A. N.

[304]—Geology.—Can any geological reader recommend a good work on the carboniferous limestone formation, which contains reliable information as to fossils; more especially as regards the North Wales and Shropshire rocks?—A. N.

[305]—The Calculus.—What books should be read on the calculus after Todhunter and Williamson? Is Price's "Infinitesimal Calculus" a good book?—Mathematicus.

[306]—Scholarships at Cambridge.—Are any open to candidates over twenty years who have not already entered?—Mathematicus.

[307]—Descriptive Geometry.—Required, a work on this subject, sufficient for a first class in fourth stage mathematics, that is, fulfilling the following syllabus:—Representation of points, straight lines, and planes, by projections and traces on two orthogonal planes. The use of auxiliary projections and rabatments. Graphic solutions of problems concerning straight lines and planes, their intersections, inclinations, &c. Problems on trihedral angles. Your paper supplies a want in educational literature.—Self-Taught.

[308]—Quicksands.—Why do persons sink in quicksands deeper than they would in water, and why can they not float as in water? Does the sand in a quicksand float in the water, and how is that compatible with the relative specific gravities of water and sand? If not, why does not the sand sink in the water?—John R. West.

[309]—Effects of Tobacco.—Will Dr. Muir Howie kindly explain why persons suffering from asthma are sometimes recommended to smoke tobacco? I gather from his interesting paper that the use of the narcotic would tend to lessen the reserve of vitality so necessary to resist the effects of violent attacks of coughing.—J. W. Brookes.

[310]—Quartz in Coal.—I have in my possession a specimen of coal to which a piece of quartz is closely attached, several layers of the same rock also running through the coal. How can this be explained?—J. W. Brookes.

Replies to Queries.

[220]—Chemical Analysis.—Might I say that before buying the new editions of Fresenius on Qualitative and Quantitative Analysis—which, as one of your readers has rightly observed, are rather too descriptive to be of use to a beginner—he should try and obtain the previous edition, which the translator cut down to about half its original volume, thereby making it as valuable to the novice as it is to the chemist. Your correspondent must not be surprised if the second-hand books should be as expensive as the new books.—F. C. S.

[258]—Drying Wild Flowers.—"H. R. S." should take to Egypt and Palestine with him a quantity of botanical drying paper (to be had of any scientific dealer), and two or more strong wooden boards of same size as paper; also strong leather straps. The plants should be spread out as naturally as possible between the sheets of paper. Then lay all between the boards, and put one or two large and heavy stones (to be had at most places) on the top. Try to keep some of the paper not in use, so that it may be changed for that in use, which becomes damp from the moisture in the plants. This changing should be made every second day, if possible. When travelling from place to place, strap the whole firmly together.—F. W. G.

[258]—Drying Wild Flowers—page 364.—I should recommend "H. R. S." to get a small 5s. book by J. L. English, entitled "A Manual on the Preservation of the larger Fungi and Wild Flowers," just published by A. B. Davis, of Epping. There is a small collection of wild flowers in the Norwich Museum preserved by this process, in which both colour and form are beautifully retained.—R. S. Standen.

[264]—Letts's "Popular Atlas Geographical Map of England and Wales" (with leading railways), will give "Strata" the information he asks for. Also Professor John Phillip's "Geography of Oxford and the Valley of the Thames" will be of great assistance. A very bright and clear geographical map (of England and Wales) is prefixed to Mr. H. B. Woodward's "Geology of England and Wales." Letts's map is a very cheap one, and is the work of Mr. Bristow; it is founded on Murchison's map. Should be glad to help "Strata" in any way I can.—Jaciebat.

[267]—Thoracic Integrity.—The statement in "Science for All" is perfectly authentic. It is really not correct to regard the chest cavity as being air-tight, as, of course, can at once be under-

stood when we think of the ease with which air can be drawn into and expelled from it. The lungs are to be regarded as elastic bags which are not connected with the walls of the chest, both they and the walls being covered with a smooth membrane called the *pleural membrane* (which, when inflamed, produces pleurisy). Now, the space (or rather separation, for they are nominally in contact), between the two layers of pleural membrane, *is air-tight*, so that when the chest cavity enlarges (by movement of ribs and descent of midriff, or diaphragm), the elastic lungs follow its walls, and so air rushes in through the wind-pipe to fill the largar space so produced, and we are said to inspire. If, however, a hole be made into the space (or separation) between the lungs and chest wall, then, of course, when the chest cavity enlarges, the lungs will not expand, but air will simply rush in at the hole that has been made. But the space between one lung and the chest-wall is quite distinct from that between the other lung and the chest-wall, so that if only one be opened into, the man still lives and breathes with his other lung; but if both spaces be opened into death must at once occur. Now, the heart is, as it were, quite distinct from these air-tight spaces, so that if a hole exist (as has been known in certain cases) through the chest-wall opposite that organ, it will not open into either of the spaces, and so will not interfere with breathing.—F. W. G.

[268]—PHOTOGRAPHY.—"Anon" has need to be careful with meddling with photography. It is an expensive pastime. For general work (landscape and portrait) consult Abney's "Instructions in Photography" (2s. 6d., Piper & Carter), or Hughes' "Photography" (1s., Simpkin). If only for portraits, Heighway's "Practical Portrait Photograph" (1s., Piper & Carter) will best suit him. Full instructions and chemicals required are given in above works, and the advertisements will indicate where to get them. The "cheapest articles" are not to be advised. Failure and disgust are sure to follow the use of cheap and necessarily nasty articles. Better give a higher price and get a good article. If it is intended to use dry plates, Eder's 'Modern Dry Plates" (3s., I think, published by Piper & Carter) will be useful.—F. M.

[269]—Carbonic acid was shown by Calvert to be necessary to the rusting of iron, besides oxygen. Sea-water contains more carbonic acid than average fresh water.—C. T. B.

[277]—Balmain's luminous paint is calcium or barium sulphide, made by heating either gypsum or heavy-spar with coal.—C. T. B.

[285]—SCIENTIFIC TERMS.—"Prester W." will find Dunman's "Glossary" (Griffiths & Farran), a useful book for "Biological, Anatomical, and Physiological Terms."—CHAS. W. DUCKWORTH.

[300].—The question of "G. G. D.," No. 300, p. 388, is a very fair one. Assuming that the evaporation from the damp surroundings have saturated the warm air inside the room, a condensation of half its vapour would certainly take place if it were reduced from 50° to 32° without any interchange with the outer air, *i.e.*, if the room were air-tight and cooled exclusively by conduction through the walls. The damp surrounding would then simply recover all the moisture they had previously supplied to the warm air. If, on the other hand, the doors and windows were thrown open, and the room were rapidly cooled by an exchange of cold air from outside, the walls would continue for some time warmer than the incoming air, and therefore would receive no deposition of moisture from it; but on the contrary, would communicate some heat to it, and thus have a drying action. I have imagined these opposite extremes as exaggerated illustrations displaying the principles. Practically, however, a mixed action occurs. Conduction takes place through the thinnest element of partition, the window glass, and there we see an abundant formation of what I may venture to call domestic dew. Besides this, an interchange of atmosphere slowly takes place; but not more slowly than the cooling of the walls. Under these conditions, the condensation of excess of vapour is limited to the window panes, and the brick or stone walls, &c., remain dry.—W. MATTIEU WILLIAMS.

[Letter 220]—HEALTH OF NAVVIES.—Edwin Sachs (p. 325), in reply to my letter on the above (p. 254), says that the "natives" are dying by thousands in Java, &c., from *fever* "though inveterate betel chewers." It would have been more satisfactory if Edwin Sachs had informed the readers of KNOWLEDGE whether the *fever* was indigenous or not, as this would, of course, make all the difference, and would account for the, at present, remarkable fact of the "natives being more easily affected by fever than are Europeans." If Edwin Sachs will read my letter again, he will see that I was alluding to fevers, &c., incidental to marsh life. Mr. A. H. Church, in his edition of "Johnson's Chemistry of Common Life," never intended the betel nut to be used as a preventive against fevers which contact with foreigners may have introduced.—F. C. S.

Answers to Correspondents.

*** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—*1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. No queries or replies savouring of the nature of advertisements can be inserted. 4. Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge. 5. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 6. Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

MARK H. JUDGE. Thanks for report of Sanitary Association, but readers would not allow us to insert so much in way of report.—E. LUXMOORE. Many thanks for description of compound pendulum, which shall shortly appear. The curves are very interesting. Is there no ink, either black, scarlet, or orange, which could be used? If there were, the curves could be photozincographed.—COGITO. When, in No. 11, I spoke of the ecliptic or plane of earth's path as unchanging, I meant that if an observer were supposed to watch the earth from the sun's centre, her apparent course among the stars would be unchanging. The earth's polar axis remains inclined at almost an unchanging angle to the plane in which the earth travels, just as the axis of a spinning top is inclined at almost an unchanging angle to the horizon plane, but this axis varies in direction (also as the axis of a reeling top varies) and the plane of the equator, which is always at right angles to the earth's axis, of course varies with it. Refer to the same figure p. 219, No. 11; here EE corresponds with the earth's equator. It remains always inclined at the same angle to the vertical, but reels round as the globe spins. I have not yet done with precession, but every week I get some such message as this: "We have forty-seven columns of over-matter, and two, three, or four more pages of advertisements than we can get in without displacing something. What are we to do?" and I have to reply, Keep out my article on comets, or on precession, or on the Pyramid, or Foster's articles on illusion, or articles by some one else who is willing (I know) to wait.—J. R. CAMPBELL. We should have liked to use your paper on the Slide Rule, but what can we do? A wants more about microscope; B more about chemistry, C botany, D entomology, E says turn out whist, and have in mineralogy, F says we want no chess, but would like something about pottery, G would like more biology, H palæontology, J says "All work and so little play makes KNOWLEDGE dull to day. Why cut down whist and chess to a column each?" and so on, to Z. You catch the idea?—M. HILL. Neither drawing represents the real changes of the moon's apparent position. If you attach a circular disc to a celestial globe, set to latitude of London, and carry the disc round from horizon to horizon, you will see the real changes, and also why they occur. A gas is said to be in the critical state when the pressure and temperature are such that, if the latter were in any degree lowered, the gas would liquefy.—F. W. B. BOUVERIE. Fear we could scarcely find room in KNOWLEDGE just now for papers on philological subjects.—J. H. FALLON. Know nothing about the Society for the Promotion of Scientific Industry.—W. B. Are not -3 and -2 as obviously roots of the modified equation?—S. STANIER. Power cannot be 200, if Jupiter had at the time of observation apparent diameter 45″. Three days before date of your letter, Jupiter's diameter was only 36″.—PRIVATE STUDENT. Your examples are rather too commonplace to be dealt with in Mathematical Column. (1.) The equations to lines parallel to $y = mx + b$, and at distance a from origin (rectangular co-ordinates), are $y = mx \pm a\sqrt{1+m^2}$. (2.) If AB is parallel to $x \cos. a + y \sin. a = p$, and $OA.OB = c^2$, equation to AB is $x \cos. a + y \sin. a = \frac{c \sin. a}{\sqrt{\tan a}}$. (3.) If AB be parallel to $y = mx + b$ and $OA + OB = c$ the equation to AB is $y = mx + \frac{cm}{1+m}$. But you really must not ask such questions as these; you mistake the purpose of KNOWLEDGE in doing so.—HISTORIAN. Your questions hardly suited to a journal of popular science. Considering how those two historians have denounced each other, it would be unwise of us to pretend to decide between them.—W. A. S. Your reply states the matter clearly, but does not explain. Mesmerism may be "nerve force producing artificial somnambulism on a susceptible subject." But why? or (if we cannot have Why) how?—J. W. WOOD. Your reply scarcely explains much.—H. W. B. Thanks; but fear the merely verbal contest you invite would be of small benefit.—GEO. ST. CLAIR. The star Alpha Centauri; moreover, this is the only first magnitude star which ever has shone in the direction of the Southern passage. The Great Bear was regarded

even in much later times than those you mention, as the polar constellation, except by the Phœnicians, who took the smaller circling of the Cynosure for their guide. The motion of the apsides in no way affects the precessional period.—T. W. Hope one day to give such an article.—W. ST. C. BOSCAWEN. A paper on Chaldean Festivals only delayed. "Omittance is no quittance."—SPECULUM. We can, however, live in air rarefied enough to support combustion so ill that a bad fire would go out in it.—C. T. B. Nay, they do not neutralise each other; the parts illuminated on the slant look less bright, emission diminishing as cosine of angle with normal to surface. Taking shot for medicine must, we agree with you, be a dangerous practice, though not necessarily *very* dangerous. Luckily, the *cæcum* does not get all it might get, or scarcely a child would grow to maturity. —UNSATISFIED. The pressure upwards is equivalent to weight of a column of air as large as mouth of glass, and 100 miles or so high (reaching, in fact, to the limits, if such there be, of the atmosphere); the downward pressure is that of the wine-glass full of water. A column of water about 39 feet high can be supported in this way, a column of mercury about 29 inches high: so that you might fill your wineglass with mercury, if suitable substance were placed over it.—JUMBO. The fancy is a queer one. I can see that it might prove very useful in keeping account of numbers. Like you, I have the numbers in sets from 1 to 12, then to 20, then to 30, then to 50, but after that I do not go straight to 100 but stop at 80. They do not, however, go in a bent line, as with you, but in a set of verticals.—W. A. F. Have never heard that the Jordan was for a time stopped up by masses of rock falling down, but as half Niagara was once for a while stopped (so they say), it seems conceivable that the same might happen to another river. Hope your question is not meant to infringe our rule about science and religion.—F. HEPBOURN. You could only now obtain Hutton's "Recreations" at a secondhand bookshop (unless, perhaps, our exchange or sixpenny sale column might help you.—EXCELSIOR. Oouff! here comes a long one! First, you are quite right. The objection was first advanced by Tycho Brahe: the answer is that the orbit of the earth is so utterly insignificant compared with the distance of the fixed stars, that no such effect can be recognised, except in the case of the very nearest stars, and then only with the most powerful and closely-measuring telescopes. Thus, Alpha Centauri, being only at the trifling distance of 20,000,000,000,000 miles, describes an ellipse having a major axis nearly 2 seconds of arc in length, one-930th of the moon's apparent diameter. 2. Twinkling of fixed stars due to moisture in our air. 3. Do not know why expeditions are not made now to South Pole; conditions there probably not similar to those at North Pole — more ice, Maury says more land. It is also a longer journey, except from places in southern hemisphere. Uranus is now very favourably situated for observation, and visible to naked eye. 4. The satellites of Mars are utterly beyond the range of a 3-inch achromatic. 5. Spots oftener seen than not, except at the time of minimum, when sometimes for months together none are seen. 6. Cannot show planet's paths now the whole sky shown in a single map. For reasons we are obliged to keep the block free from anything not belonging to the stellar heavens. The zodiacal map does better in every way. To see Uranus or Neptune to the least advantage, a good telescope is wanted, and if a map is trusted to show where the planet is, it must be on a much larger scale than our star map. This applies also to the nebulæ, except those visible to the naked eye. Am glad to hear you have been so thoroughly well pleased with the telescope you obtained from Mr. Bateman for the small price of £5. 15s., "being and doing all that is stated of it in the advertisement, and more too."—ANXIOUS ONE. If there is the danger you fear, it is far too serious a matter to be trifled with. We should be wronging you if we inserted your letter for casual reply, and we have no right to ask medical men, especially mind doctors, to discuss the matter. Indeed, we know they would not do so (because *they* know they could not properly do so) without careful study of the case. Pray believe that if we could do anything, rightly, by which your anxiety might be relieved, we would most willingly do so. One thing we can say—the symptom you describe should either lead you to seek medical advice, or you ought not to allow it to worry you at all. A doctor would probably tell you how much you should notice it, and with what object. But merely noticing it, and being worried by it, can do nothing but harm.—J. O. LINDSAY. Thanks for extract. The publishers will probably issue covers for binding. A yearly volume would be too thick. Taking twenty-two pages per number, without advertisements, there would be 1,144 pages; far too many for a single volume.—JAMES DICKINSON. Thanks for the microcrosm; but the article on Sound is written by one who understands not.—J. V. M. You seem to misunderstand my remark, that one of our contributers was "not writing for" a correspondent (who objected te his reasoning). No sneer as to that correspondent's capacity was intended. I trust I am incapable of such rudeness. I meant simply what I said, that Dr. Wilson's reasoning was not directed to meet the particular objection urged by that objector. You think scientific men belaud each other too much. Is not that better than belittling each other? Would you have them like the Professor of Greek, who wished an opponent "'confounded' for his theory of the irregular verbs"? I cannot admit that Dr. Draper did not argue honestly. He was a hard hitter, but honesty itself. The case you cite is not very serious. Draper says Pilate was not answered, and Bacon said Pilate would not stay for an answer; do you know which opinion was right? You go on to object to stories about animals. You say, What would scientific men say to evidences of religion consisting of stories of pious men. Our stories of animals are not meant as evidences of science. Then you think our articles too short. But others want variety. Your letter *does* seem rather wandering, but doubtless "the gaps are caused by suppression before birth of much that you wished to say."—E. W. P. Both papers received. Thanks. Great pressure of matter only has prevented our using them yet.—R. W. J. Statement about tides in *Christian Globe* utterly wrong. Scientific authorities differ as to details, of course; but none support the ridiculous statement you quote about "geological time approaching the limits," &c.—M. H. JUDGE. Sorry; but letter reached us only when No. 18 was already in type.—SIRIUS. Statement in *Times* probably near the truth. From best estimates, Sirius gives out about 300 times as much light as sun (at same distance), which would give surface 300 times as great (if of same intrinsic lustre); diameter about fourteen times as great. Of course, this is but an estimate.—F. MATIN. Thanks.—A. A. FERGUSON. Smoke and fog best *seen* where light was, in cases cited; not attracted by light. The theory impossible.—C. A. C. Theory not reconcilable with evidence of former existence both of water and air on moon.—CONSTANS. In such a subject, Dr. Ball was obliged to assume either that readers were acquainted with those laws, or would take them for granted. Your questions chiefly relate to the more doubtful and perplexing matters. You may see from my article in *Contemporary Review*, and hereafter more fully in *Gentleman's Magazine*, that there is room for considerable variety of opinions as to details.—G. S. Thanks; but answer about lightning too vague.—W. CRISP does not consider the evidence of Mallicolese skull so decisive against phrenology as Miss Buckland suggests (in passing) that it is; convolutions not stunted in growth, but forced in other directions. He notes that capacity of Neanderthal skull cannot be estimated, as it is a mere skull cap.—J. F. LAWRENCE. Should advise you not to use copper bowl for lemon-squeezer until thoroughly retinned.—F. NIMR. Hope you will occasionally send *translated* extracts from Al Muktataf. Of course, I understand Arabic perfectly, but "faites comme si je ne le savois pas," as M. Jourdain says.—AN ENGINEER writes pleasantly-worded note explaining that "F.R.A.S.'s" remarks about ice had seemed to him insufficiently clear.—A. R. SENNETT. Do not know; *cannot* answer correspondents per letter.—TARANAKI. We may hereafter publish some *southern star-maps*, but at present our hands are full. You see we italicise words in question, at your suggestion, and may hereafter adopt the system, if printers do not object.—F. F. PORRELL. Red Sea was once thought to be higher than Mediterranean; disproved first by measurement, then emphatically by canal.—H. L. MACQUARD. Nos. 2 and 3 quite out of print; Nos. 1, 4, 5, 6, 7, 8, and 9 not very far from it, or might reprint 2 and 3; as it is, it would be useless.—F. C. S. Will try soon to find room for "Wood Gas," but "Beet" is waiting. In fact, we are choked with matter at present.—ALEX. F. If you read Sir J. Herschel's essay on "Light" ("Familiar Science Essays)," you will see what a very difficult question yours is.—A. W. BUCKLAND gives for benefit of "Experto Crede" address Professor Rudler, Jermyn-street Museum, for information about mineralogical objects.—A. J. D. saw cat on sill of fanlight over door, who presently lowered his hind legs, and, lifting knocker, let it fall, walking in when door was opened.—DALETH, T. A. PRESTON, R. MACPHERSON, and others. Thanks, but query already answered when your reply came.—W. MIDDLETON BUTTERS says in "Pig and Whistle" a pig = a cup or bowl, and whistle = wassail; "Bear and Ragged Stump," crest of Warwick, the King-maker; in "Magpie and Stump," mag = large coin, guinea; pie = pay; and stump = be off.—J. T. B. Ere long electrical matter will be dealt with as fully as possible.—LEO. H. WALKER. Afraid to publish your suggestion, lest some mother should be tempted to squeeze "baby's" brain to make a chief of him; what a terrible thing if a father should find mamma or "nurse" with copy of KNOWLEDGE before her carefully trying to squeeze baby's head into semblance of young Mallicolese chieftain!—AMATEUR BOTANIST. Questions about preparing colours for magic-lantern slides and plant collection require articles to themselves. — MISS J. YOUNG. Thanks, but we have a surplus of original stories.—ARTHUR E. PALMER. Thanks. I ought to have added that when I am pressed,

and have to work my very hardest, I always go without stimulants of any sort. In ordinary working time I am a very moderate drinker; in holiday time, like Mr. Foker, I "take my whack" with the rest; but then I do not believe in holiday-making; it means, with me, "getting out of working order."—F. A. B. Your first query a statement—namely, that coronal ring was perfect round moon at first quarter. Other query inserted; letter (abstract) also. Thanks.—SIR H. THOMPSON. I take some blame to myself, for I had read your book, and remembered well that there was therein no advocacy of vegetarianism.— M. S. Thanks. — E. G. D. Your "more than thirty notes, criticisms, and suggestions" came upon us all "too too" much at once, and many related to matters already, as we hoped, disposed of. Others have had more to complain of than yourself; but if what you wished done for you were done for all, we should want sixty pages weekly, and the paper alone would cost much more than our weekly price. What should we do? Advise proprietors to raise price? Thanks; we prefer not, if by any possibility we can avoid it. But, as you will have it so, we reply, "Farewell." Try to be a little more reasonable with the next periodical you take.—E. V. H. Fear you cannot get a really good account of the comet of 1843; the best that was written about it lies buried in proceedings of astronomical societies. The "meteoric theory of comets" (scarcely a theory now) has been dealt with fully by several writers, myself among others. It is to be considered shortly in these pages.—E. BURKE. The version given in the work you mention long since disposed of by Leverrier. —J. H. COBBETT. I think both Parallax and Mr. Newton Crosland would feel insulted at the suggestion that they are one and the same. If either could destroy accepted astronomy, the other would fall upon him. In the theory of Parallax (who is by no means the same as our too lively Hampden) the earth is not compared to a Stilton cheese more than to a Dutch cheese. The earth has only one side—the top; the north pole is the centre; there is no south pole, but in its stead we have the circumference. Dimensions I do not know. "Hampden" tells me one thing; "Parallax" used to assert another. If you quote either, the advocate of the other—whether "Hampden" or "Parallax"—tells you you know nothing of the Zetetic astronomy. If that advocate chances to be "Hampden," he calls you a lying coward, or a lily-livered, perjured villain, or something of that sort. It is a way he has. "Parallax" is very different. He is not only gentlemanly, but he is "like Cerberus, three gentlemen at once." At least, to my certain knowledge, "Parallax" was Mr. Rowbotham in 1864; De Morgan says of him ("Paradoxes," p. 306,) that at Trowbridge, in 1849, he was S. Goulden; and now he is Dr. Burley.—J. MURRAY. Yes; other notes and diagrams (gracious goodness!) received. Sorry "the Ptolemaic Decrescendo system will not allow any spots on the sun," only allowing them to pass in the same way as Venus. Astronomers are far kinder.—J. A. M. Solutions 1, 2, 3 received; hope with you, the pork is thoroughly cooked now.—R. P. T. Gregory's Electrical Theory plausible as you say, but, as you also say, quite irreconcilable with Dr. Ball's views; equally irreconcilable with laws of dynamics.—CARUS. The more hydrogen in a balloon—the hydrogen being enclosed in elastic case, so that it is nearly at same pressure as surrounding atmosphere—the greater the lifting power; otherwise, the reverse. If an air-tight case is so made as to be of constant dimensions, the more hydrogen you force into it the less will be the raising power. As to the other query, please specify the kind of work you require on palæontology—technical, popular, or what?—ELI WALLIS. If you want to see what stars lie towards, say, the south west, hold the map so that the words south-western horizon are vertically below the map's centre, then between the south-western boundary and that centre, which represents the point overhead, you will see in the map the stars you want.—FARMER. Letter marked for insertion.—J. A. OLLARD. Not a tenth of the space you want is available.—GERALD MASSEY. Thanks; but question of dog's descent is rather a biological than a philological one.

Letters Received.

H. Muirhead, W. H. Morgan, Aspiring Artist, J. Hartington, L. M. N., K. Mongar (?), M. Emerson, P. T. L., Audax, Peter Parley, Sucking Herschel (must not suck brains), E. F., Materfamilias, Peterkin, Excelsior, J. North, M. Weatherwit, St. Pancras, Q. E. D., F. Y., Formosa, Empty Noddle (try to fill), James Ingersoll, Amory, N. C., Philip St. John, Northern Lad, Calais-Douvres, Amplitude, N. Tressingham, Cœlebs, Shingly Beach, M. Peterson, J. Short, &c., &c.

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Our Mathematical Column.

THE LAWS OF PROBABILITY.

BY THE EDITOR.

THE mathematical discussion of the laws of chance is regarded by many with suspicion, because they observe that while the matters discussed are admitted by the very inquirer to be doubtful, the conclusions arrived at are presented as matters of mathematical certainty. But in reality this arises from a misapprehension of the nature of the inquiry made by mathematicians into questions relating to chance. A mathematician assigns a definite value, as if it were certain, to the chance of winning a prize in a lottery (where one prize only, let us say, can be won) under given conditions; but he does not assert that the event will confirm his opinions; on the contrary, he knows that whatever happens, the sum he names will not be gained. He says *that*, certainly, is the value of the chance, but he knows that either the prize will be won, in which case more than the sum he named will be won, or lost, in which case the drawer of the blank will win nothing. He cannot even say that in any given number of trials the average amount won will be what he has named; he can only say that the greater the number of trials, the nearer will the average amount won be to the amount he has named. On this point only he is certain, and not only can his view be shown by logical reasoning to be sound, but multiplied experience confirms it. The reasoning may not admit of being grasped very easily, or, at any rate, very quickly. In particular cases the mathematical determination of the value of a chance may be so difficult that only advanced mathematicians can master the demonstration. But even in such cases, experiments can often be made quite easily, by which, with a little patience, the mathematical solution may be shown to be correct. Take, for instance, one of the "chance methods" of squaring the circle. A straight rod of given length, and of given square section, is tossed at random on to a grating of equidistant bars, and after gyrating in the air a number of times, falls either athwart the bars or between them, according, one would say, to pure chance (or bare chance, or mere chance, as you may choose to call it). A mathematician says that the chance of the rod falling through—the spaces between the bars being, of course, wider than the rod—depends (in what seems an occult fashion) on the relation between the circumference and the diameter of a circle. The proof is not simple, and perhaps you fail to understand it. But set some one to toss the rod (from a place where he cannot see the cross-bars, and without any knowledge of their position) a few thousands, or tens of thousands of times, and note how often it falls through, and how often it fails to fall through; you then find that the ratio of the two numbers approaches very nearly, the more nearly the oftener the rod is thrown, to the ratio assigned by the mathematician. The experiment may be tried any number of times, and always the result is the same.

The science of probabilities is shown by such instances as these to be a science which can predict, even in matters of pure chance. It is not a science which authoritatively lays down certain *dicta*, but one which itself indicates ways in which it may be put to the test.

But then, say objectors, probability is dealt with by mathematicians in so artificial a manner, that these methods cannot possibly have any application to real events. At the very outset there are conventional rules, which, so far as we can judge, might just as well have been entirely different.

In reality, however, the rules by which mathematicians deal with probabilites are only conventional in the same sense that it is conventional to measure lines by inches or by feet, to measure angles by circular arcs, or to measure surfaces, solids, time intervals (what you please, in fine, that mathematics can deal with) as mathematicians do measure these quantities.

Let us see what these conventions are:—

In the first place, it is agreed that absolute certainty shall be represented by unity, absolute impossibility by 0, and therefore (necessarily) different degrees of probability by different proper fractions. We can thus never have a chance greater than 1, for nothing can be surer than the sure; nor can we have a chance less than 0, that is negative, for nothing can be more impossible than the impossible. These are pure conventions. We might have called certainty 10 or 100, or $59\frac{1}{15}$, or anything we pleased; we might equally have represented impossibility by any quantity, positive or negative, or either certainty or improbability by a letter. It is found convenient, however, to adopt the particular convention mentioned, and so long as, having once adopted it, we uniformly follow it, we shall no more be likely to go astray than when we represent the number "three" by the figure three throughout an arithmetical sum.

These conventions are in reality all which the student of probabilities has to make; all others which seem to arise, as his study of the subject proceeds, are in reality only necessary consequences of these.

Let us now consider some simple illustrations of the results of these conventions.

Suppose a bag contains ten balls, all white. Then the chance that a white ball will be drawn is unity, and the chance that a black ball will be drawn is 0.

If all the ten balls in a bag are of the same size, and the drawer can as readily reach one as another, or if the bag is well shaken, so that each ball has an equal chance of coming to the top, the chance of any given ball being drawn at a given trial must be exactly equal to the chance of any other. All the balls are equally likely to be drawn. So that if C be the chance for each, $10\,C$ is the chance for all the ten. But one of them must be drawn. Hence $10\,C$ must be equal to certainty, *i.e.* to 1; and $C = \frac{1}{10}$.

This is obvious enough, but let us submit it to a test.

Suppose one ball is white and the remaining nine balls all black, and suppose that if the white ball is drawn, a certain prize, say £10, will be won. Now, if there are ten persons to draw, and each takes one ball, it is certain that one of them will draw the white ball. But there is nothing to prevent one and the same person buying up the chances of all the ten, or the certainty of drawing the white ball, and so winning the prize of £10. He should clearly pay the same sum to each, for each has the same chance of winning, and the total sum he should expend should be clearly £10, for he makes certain of getting that sum by buying up all their chances. Since, then, he has to pay £10 in equal sums to ten persons, he must give £1 to each. This, therefore, is the value of each person's chance of drawing the white ball, and this sum is one-tenth of the sum at stake. This is the same as saying that the chance of each is $\frac{1}{10}$.

We have here spoken of a bag of 10 balls; but it is clear the reasoning applies to any case in which there are a number of events all equally likely, and one of which must happen. If there are 13 balls in a bag, the chance of drawing any particular ball is $\frac{1}{13}$. If there are n, the chance of drawing any given ball is $\frac{1}{n}$. If a die has all its six faces numbered differently, the chance that when the die is thrown any given number will be uppermost, is $\frac{1}{6}$, if the die is not loaded,—that is, if it as likely that one face will be uppermost as another. Again, if a coin is tossed with a good high spin, and the coin is uniform, so as to spin with perfect freedom, the chance of head or tail is $\frac{1}{2}$. So the chance of drawing any particular card from a picquet pack of cards is $\frac{1}{32}$; the chance of drawing any particular card from a whist pack is $\frac{1}{52}$; and so with other such cases.

Next, suppose that a bag contains three white and seven black balls, or ten in all, and let us inquire what is the chance of drawing a white ball.

The chance of drawing any particular white ball is $\frac{1}{10}$. It might seem, then, sufficient to reason that, since there are three white balls, and the chance of drawing each is $\frac{1}{10}$, the chance of drawing any one of the three is $\frac{3}{10}$. The reasoning is, indeed, just, and the result is correct, but the student cannot be too careful in avoiding all hasty conclusions in these questions of chance. We shall see presently that a line of reasoning which seems at first sight quite as just as the above will lead to an obviously incorrect result. It will be well, then, were it merely to initiate a system of close inquiry into these matters, to discuss the above result somewhat attentively. We shall save time in the long run by getting these seemingly axiomatic matters thoroughly reasoned out.

Applying our former method of measuring chances by considering the value of the right to draw tickets in a lottery, we readily determine the chance we are seeking. We have only to suppose that there are three prizes equal in value—say each £1. Then the chance of each of the ten drawers must needs be equal. Now, anyone who bought up all these ten equal chances should clearly pay £3, since this is what he would obtain when the ten tickets were drawn. Each ticket would therefore cost six shillings—that is, the value of the chance of each drawer is $\frac{3}{10}$ of the prize gained by a successful drawing. We infer the justice of the conclusion that the chance of drawing one white ball from a bag containing seven black and three white balls is $\frac{3}{10}$.

Since the above reasoning is applicable, whatever the total number of balls, and whatever the number of white balls, we have this general result, that the chance of drawing a white ball from a bag containing n balls, of which r are white, is $\frac{r}{n}$.

Further, since the reasoning is as applicable to the black balls as to the white, it is obvious that the chance of drawing a black ball out of a bag containing seven black and three white balls is $\frac{7}{10}$. This is the chance of *failing* to draw a white ball. And, generally, the chance of failing to draw a white ball from a bag containing n balls, of which r are white, is $\frac{n-r}{n}$.

Nor is the reasoning affected if the balls which are not white are of more than one colour; while the same reasoning applies to the balls of divers colours. So that we clearly get this general rule, including all that we have thus far attained to:—If there are n balls (all equal in size) in a bag, of which w are white, b black, r red, g green, and so on, till all the colours and balls in the bag are reckoned, then—

The chance of drawing a white ball is $\frac{w}{n}$

„ failing to draw a white ball is $\frac{n-w}{n}$

„ drawing a black ball is ..... $\frac{b}{n}$

„ failing to draw a black ball is $\frac{n-b}{n}$

„ drawing a red ball is ..... $\frac{r}{n}$

„ failing to draw a red ball is ... $\frac{n-r}{n}$

and so on through all the colours, and we may also combine any of the colours together in such statements as the following:—

The chance of drawing either a white or a black ball is ..... $\frac{b+w}{n}$

The chance of failing to draw either a white or a black ball ..... $\frac{n-(b+w)}{n}$

and so on

This general law is applicable to any case where the chance is required that one of a certain set of events will happen out of a larger number of events whose chances are equal. Thus, the chance that either ace or deuce will be thrown with a single die is $\frac{2}{6}$. The chance that a court card will be drawn out of a picquet pack of cards is $\frac{12}{32}$, the corresponding chance in the case of a whist pack being $\frac{12}{52}$.

[Solutions of problems, by T. R. and others, in our next.—Ed.]

A Substitute for Water in Foot-Warmers.—People who travel much in winter, either in railway carriages or in any of the other modes of conveyance, are continually annoyed and inconvenienced by the fact that the hot water in their tins gets cold very soon; in fact, if the tins are to be of any comfort to the traveller, they must be changed every two hours. Who, amongst all those who have felt starved and miserable through the water in the foot-warmer having become cold, will not hail with delight the fact that the science of chemistry promises speedily to bring a fresh boon to the traveller in all parts of the world, in the form of a foot-warmer which will keep hot for a period of ten hours, at the same time giving out four times as much useful heat as water? The name of the chemist who is bringing about this great and useful change is M. Angelin, who purposes using crystallised sodium acetate instead of water. When once the tins are filled, the stoppers well soldered, and the warmer perfectly air-tight, all trouble ceases except warming them up when required for use. The tins can be used over and over again, the salt being perfectly stable. Experiments are now being tried on the London and North-Western Railway, also on various foreign railways, with a view to its adoption.—F.C.S.

Our Chess Column.

Games between Mephisto, and a strong Amateur.

Allgaier Gambit.

WHITE. MEPHISTO.	BLACK. MR. M.	WHITE. MEPHISTO.	BLACK. MR. M.
1. P to K4	P to K4	14. P takes P	Q takes P
2. P to KB4	P takes P	15. B to Q3 (ch)	K to Kt2
3. Kt to KB3	P to KKt4	16. Castles KR	R takes B(c)
4. P to KR4	P to Kt5	17. P takes R	Q to B4 (ch)
5. Kt to Kt5	P to KR3	18. R to B2(f)	Kt takes P(g)
6. Kt takes P	K takes Kt	19. K to Rsq	B to K 3
7. P to Q4	P to Q4	20. R to B6	Kt takes R(h)
8. B takes P	Kt to KB3	21. P tks Kt (ch)	K takes P(i)
9. Kt to QB3	B to Kt5(a)	22. Q takes P(ch)	K to K2
10. B to K5	B tks Kt ch(b)	23. R to Ksq	Q to Q4
11. P takes B	R to Ksq	24. P to B4	Q to Q3
12. Q to Q2 (c)	K to Kt3(d)	25. B to B5	resigns
13. P to R5 (ch)	K to R2		

(a) The best defence is to employ the Bishop on K2 in combination with R to KBsq and K to Kt2

(b) The tempting move Kt takes KP would not be good play.

(c) Or B to K2

(d) Black might have defended with 12 Kt to QB3, 13 Q takes R P, 13 Kt takes B, 14 P takes Kt, 14 R takes P.

(e) A powerful resource, which threatens to break up White's game.

(f) A desperate move, but if K to R2 or Rsq, then by Q takes KP Black would completely domineer over White's game. Of course, White cannot afford to exchange Queens by interposing her on B2. R to B2, besides proving an effective defence, also keeps the attack in hand, for it would not be quite so good now for Black to play, Q takes KP, for White would then reply with R to Ksq, and Black could not then take the RP with a check, which would have been the case had the King gone to Rsq or R2. The winning of the time of one move won the game for White.

(g) Black dare not play P to Kt6, for then White would reply with P takes Kt ch, and dissolution would speedily follow.

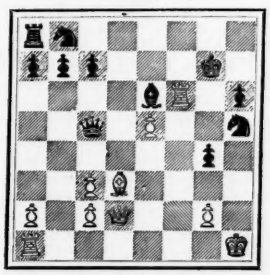
(h) White not only threatens R takes B, but also Q takes R P, Black has no alternative but to take the proffered Rook. The sacrifice is perfectly sound.

We give a diagram of the position.

Position after White's 20th move:—

AMATEUR.

BLACK



WHITE

MEPHISTO.

(i) K to B2 instead of K takes P looks better, but it would also lose, *e.g.*:—

21. K to B2

22. Q takes P / Kt to Q2 or K to Ksq

23. B to Kt6 (ch) / K takes P — R to Qsq / B to B2 or Kt to Q2

24. B to B5 (ch) / K takes B (a) — B to Kt5 (ch) / P to B3 — B to Kt6 (ch) / K to Qsq

25. R to Bsq (ch) / K to K4 — Q to R8 (ch) / Q to Bsq — Q to R8 (ch) / and mates in two moves

26. R to Ksq (ch) / and wins — R to K8 (ch) / and wins

(a) 24. K to K2 — 25. Q takes B ch / K to Bsq — 26. Q takes Kt / and wins

GAMES BY CORRESPONDENCE.—(*Continued from p. 396.*)

GAME I.

CHIEF EDITOR.	CHESS EDITOR.
	3. P to Q4
4. P to K5	4. P to QB4
5. Kt to KB3	5. P takes P
6. Q takes P	6. Kt to QB3
7. Q to B2	7. B to QKt5 (ch)
8. P to QB3	8. B to QR4
9. B to Q3	9. KKt to K2
10. P to QKt4	

GAME II.

	3. Q to B3
4. P to Q4	4. P to Q3
5. Kt to K B3	5. Kt to QB3
6. P to Kt3	6. B to Q2
7. P to K5	7. Q to K2
8. P to QR4	8. Castles
9. B to QR3	

SOLUTION OF PROBLEM NO. 20, p. 350.

By J. A. MILES.

1. B to R4 (ch) — R takes B
2. Q to QR7 — K takes R (a)
3. Kt to K6 dis. ch. and mates next move.

(a) If R takes QKt to K8, mate.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

J. A. Miles.—Problems received with thanks. Your kindly-proffered assistance welcome. We can only benefit by your experience.

E. C. H.—Yes.

J. S. Flower, Ryde.—Received with thanks, but too obvious for insertion.

D.—Self mate is to compel your adversary to mate you.

H. A. L. S.—Problems 21 and 22 are intended to be easy. We should displease far more readers by giving difficult problems only than by giving occasionally easy ones. Curious to say, the easy problem of which you speak so disparagingly has proved too much for you; for if 1. R to K.7 (ch), then Q takes R, and there is no mate.

H. Percival, R. S. Standen, and others.—Answered last week.

F. H. Jones.—Solution of No. 20 incorrect; if 1. R takes P, 1. R to Kt7, and there is no mate in three.

Correct solution of Problems No. 21 and 22 received from G. B. T., J. Licence, A. C. Skinner, A. J. Martin, H. Percival, Henry Bowmann, R. S. Standen, Brenton, F. H. Jones (of No. 22 only), S. H. G., J. R. W.

Problem 21.—In this problem there is a Bishop on White's King's Rook square, with a Pawn on Kt2. The object of the Bishop is to command the square on QB6; if, therefore, we replace the Bishop on KRsq. by a Queen, this object is likewise achieved, and the anomaly (if it may be so called) is obviated.

PROBLEM 21, p. 369. From Squire.

White.	Black.
1. King's Knight's Pawn to his 4th	1. Queen takes Castle
2. Knight to King's 6th (from K B4 mate.	

Variation.

1. As before	1. If Queen takes Bishop
2. Castle to King 7th. Mate, &c.	

PROBLEM 22, p. 369.

White.	Black.
1. Queen to Q8	1. K to KB2
2. Kt to K7, mate	
1. As before	2. Queen to Q2
2. Queen to KB6, mate, &c.	

NOTICE.—For "Nos. 2 and 3 are *not* out of print," in Answers to Correspondents, No. 17, read "Nos. 2 and 3 are *now* out of print." Part I. is now entirely out of print. Those who wish to complete the series would do well to get the Parts which are still in print, and to add their names to the list of applicants for Part I., so that, should any copies be returned, they may be distributed in due order. No. 5, also, is now out of print.

XUM

Our Whist Column.

By "Five of Clubs."

J. HARTINGTON.—The law against revoking, like most of those which involve penalties, is intended to prevent players, either through carelessness *or otherwise*, from doing anything by which they might obtain a wrongful advantage. Wilfully revoking would be cheating, were there no penalty; it is none the less cheating when it is done with the hope of escaping the penalty, or to hide a former honest revoke. The penalty for wilful revokes is exclusion from the society of honest card-players.

Five of Clubs.

Problem 2.—An easy Double-Dummy Problem.

Colonel Drayson gives the following amusing example (which occurred to himself at double dummy) of the difference between practical and theoretical Whist. As he says, if any player had played at Whist as it is necessary to play in the following case, he would probably have been accused of trying to lose the game:—

The Hands.

A.
Hearts—10, 6, 4.
Spades—A.
Clubs—A, K, Q.
Diamonds—10, 7, 6, 5, 4, 3.

B.
Hearts—5, 3, 2.
Spades—9 8, 5, 3, 2.
Clubs—None.
Diamonds—A, Q, Kn, 9, 8.

Y.
Hearts—A, K, Q, Kn.
Spades—K, 10, 7, 4.
Clubs—10, 7, 4.
Diamonds—K, 2.

Z.
Hearts—9, 8, 7.
Spades—Q, Kn, 6.
Clubs—Kn, 9, 8, 6, 5, 3, 2.
Diamonds—None.

Score:—*A*, *B* 4; *Y*, *Z*, love.
Y Z to save (and win) the game.

Next week an interesting game, kindly sent us by Mr. F. H. Lewis, will appear, illustrating the use of the penultimate as a means of conveying information to partner.

Sir,—As "Five of Clubs" says that playing Knave second hand from Knave two, a small card having been led, would not be signalling for trumps, and that Clay says so, whereas I argued that, for all the third player could tell, the second player might be asking, and, as "Five of Clubs" has, I am convinced, somewhat misunderstood Clay's meaning, allow me, on a point of such frequent occurrence, and in which so many go astray, to justify my comment and elucidate the position. On referring to Clay, your readers will note that he is impressing the fact that to ask for trumps a player must throw away an unnecessarily high card, and he illustrates this by an example of his partner playing a ten second hand on a small card led, and afterwards playing a small card, whereon Clay says, "He thinks he has asked for trumps, but he has done no such thing. His ten is not, so far as I can tell, an unnecessarily high card. It is an effort to take the trick. It may be played in the ordinary way from Knave, ten, and a small card of the suit." Clay's meaning clearly is, that inasmuch as the ten may have been played from Knave, ten, and a small one, and would in that case not have been an unnecessarily high card, the play of the ten, and then a small one, is not *per se* asking for trumps. And he clearly implies that if he knew or could infer that his partner did not hold the Knave, he would have regarded it as asking for trumps; for remember that he expressly lays it down that, with Knave or ten and one small card second hand, the small card is to be played, unless to cover; so that, in his opinion, holding ten, or Knave, and a small one second hand, and playing ten or Knave on a small one, would be an unnecessarily high card, and that is the test. Cavendish's language on this point is not liable to the same misinterpretation as Clay's. He says, "It is important to distinguish between covering second hand and discarding an unnecessarily high card. For example, with Knave, ten, and a small one, it is usual to play the ten second hand on a small card. When the small card is played the second round, it is not a signal for trumps, unless your partner can infer that you do not hold the Knave." So equally and by parity of reasoning, a player holding Knave and a small one can by playing the Knave second hand, when a small one is led, ask for trumps, but it will not be a signal to his partner unless his partner himself holds the Queen, or can infer that it is in the hands of either adversary. And this, according to my experience, is the view adopted in play by Cavendish and other fine players. I therefore submit that in Problem 1 *B* (third player) could not be sure that *Y*, who played the Knave second hand and must hold the two, was not asking for trumps.

On the other matters at issue between myself and "Five of Clubs," I will only say that some expressions in his letter to you, Mr. Editor, give inaccurate impressions of the contents of my letter, for which you could not find space. Mogul.

If our whist readers consider a few sentences preceding those quoted by "Mogul" from Clay, and a few which follow, they will see, we think, that Clay, at any rate (rightly or wrongly), thought the play of ten followed by small one (with these two cards alone in hand) an unsatisfactory way of signalling. The passage runs:—"My partner is second to play, *and holds, say, the ten and a small card of the suit*, which the adversary opens with a small card. My partner being second player, plays his ten, and the trick is taken with the King; the lead is returned, and the original leader takes with the Ace, my partner throwing his small card." He thinks, &c., as quoted by "Mogul," "He could only have given, in this way, a *legitimate* invitation for a trump, if the card originally held had been higher than the ten, which, in this case, would have been an unnecessarily high card."

It seems to me clear that Clay, with his (perhaps exaggerated) abhorrence of small cards, here teaches that if you hold ten and a small one second hand, you should not, even though you desire to signal, use these cards for the purpose, lest your partner (not holding Knave or having any means of placing it elsewkere), should infer that it remains in your hand, unguarded. Of course, every whist player knows that with Queen and one other, or Knave and one other, or ten and one other, the small card should be played. Every whist player also is familiar with Cavendish's explanation (given in almost identical terms by Pole also) that with Queen, Knave, and small one, or Knave, ten, and a small one, the highest of the sequence should be played first if you want to signal. But that I take it is a different point, not, as Mogul opines, the same. Cavendish is speaking of a case where second hand holds three, Clay of a case where he holds only two. However, Problem 1 was not mine, but taken, as stated, from the *Westminster Papers*.—Five of Clubs.

And this leads me to note that many querists seem unable to understand how *Z* could know from *Y's* play in the ninth and tenth rounds of the game in No. 13, p. 284, that *Y* held the Knave. I was *Z*, *Y's* partner, and I knew it; the "proof of the pudding," &c. Let me explain how I knew it—at once, without having to think over the matter. *Y* usually played strictly in accordance with the familiar rules, always "following" with lowest of a sequence, unless there was special occasion to depart from the rule. In this case he manifestly had not played the lowest, for after the ten fell the nine. I knew he would never have played ten before nine, from the sequence ten, nine only. I was absolutely certain, therefore, that he held the Knave. It was the only way in which he could have shown it. He was bound to play one of the sequence; the small card would have lost a trick and the game. If he played nine, then ten, I should have known nothing about the Knave; if he had played nine, then Knave, I should have known nothing about the ten, or rather I should have supposed ten with the adversary. If he had played ten then Knave, or Knave then ten, I should have supposed the nine with the adversary. By playing ten, then nine, he showed me the position of the third card of the sequence. My own play of the King first was decidedly wrong, though, of course, strictly in accordance with rule. I felt this the moment I had played it; who has not made such mistakes? But it seemed to me at the time that *Y* hit on a very ingenious course to show me he held the three sequence, by departing from the customary rule and playing the middle card. Of course, if he had not been a steady player, I might have supposed the play of ten followed by nine a mere piece of carelessness; but I felt certain it was *not*.

Five of Clubs.

NOTICES.

The Back Numbers of Knowledge, with the exception of No. 2 (Nov. 11, 1881), No. 3 (Nov. 18, 1881), and No. 5 (obtainable only by taking Part II.), are in print, and can be obtained from all booksellers and newsagents, or direct from the Publishers. Should any difficulty arise in obtaining the paper, an application to the Publishers is respectfully requested.

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